THE

BALTIMORE PHILOSOPHICAL JOURNAL AND REVIEW.

NATIONAL LIBRARY OF MEDICINE

BY JOHN B. DAVIDGE, A. M. M. D.

PROFESSOR OF ANATOMY IN THE UNIVERSITY OF MARYLAND.

No. I.

JULY, 1823.

O curas hominum! O quantum est in rebus inane!—Quis leget hæc?

Baltimore:

PUBLISHED BY JOSEPH ROBINSON,

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PROSPECTUS

OF A WORK TO BE STYLED

THE BALTIMORE PHILOSOPHICAL JOURNAL AND REVIEW;

EDITED BY

JOHN B. DAVIDGE, A. M. M. D.

And Professor of Anatomy in the University of Maryland.

The above-mentioned Journal and Review will be supported, in co-operation with the Editor, by several Medical gentlemen, among the most distinguished and best informed of the profession; and from whose talents and exertions, every thing necessary to give character, add utility, and ensure permanency to such a work, may be confidently expected.

The authority of custom, and prevalence of example, have imposed laws on Editors; and we feel it our duty to lay before those whom we expect to be our patrons, the outlines of our pretensions and design. To secure patronage, we must awaken expectation and inspire confidence.

That Baltimore, among the most prosperous in commerce, and respectable in intellectual distinction of the cities of America, should be without a periodical work, either on general Literature or particular Science, excites astonishment. Our physicians, many of whom are distinguished for genius and acquirement, are discouraged by the anticipation of the trouble and expense of individual publication, or deterred by the prospect of appearing alone before the public eye; they pass their lives in silence, and their copious research and extensive ex-

perience lie buried and lost; or at best are made known to the world through distant channels. Thus Baltimore stands obscured, while other cities though erroneously, are regarded as the sources and fountains of science, when, in fact, they are often the mere conduits through which the streams of our learning flow. Merit is neglected because it does not appear; and modesty pines while assurance is applauded.

Other considerations, of no little weight, urge the Editor on to his undertaking. The views and doctrines of the University of Maryland are, in many instances, new and important; and it is proper that the republick of medicine be made acquainted with them. The physicians of this University took the lead, and impressed a decisive character on the great question in regard to the contagion of the Yellow Fever. Perhaps their distinction and claim may not be circumscribed to this solitary point. There are other motives which strongly suggest the undertaking. No inconsiderable number of the practisers of physick, in the more remote sections of the country, owing to the trouble and disbursements in procuring books, are debarred the pleasure and intellectual profit of perusing European publications—hence they are unable to keep pace with the march of improvement in their science. To such members of the profession the present work will afford great and numerous advantages, as it is contemplated by the Editor to make such arrangements with London, Paris, Edinburgh and Vienna, as will ensure the earliest reception of all productions of merit, on Medical and Chemical Philosophy. By such acquisition the American physician will be placed on even ground with his brethren of Europe. Indeed he will receive all that is valuable, chastened and enhanced in value, by appropriate comment and liberal analysis.

Moreover; to Medical gentlemen throughout the United States, this Journal will afford opportunities of giving publicity to their own sentiments and observations. Especially will facilities be offered to the Western and Southern districts; the more remote points of Pennsylvania and Delaware, for the promulgation of whatever may be useful in speculation or valuable in fact.

The Editor is thus particular, and indicates individual states and districts on the ground, that to the more eastern are already opened suitable and numerous channels of Literary and Scientifick intelligence.

While the Editor will receive, with gratitude and acknowledgement, any communication that may be offered within the scope of his scheme and purpose, yet he retains to himself the liberty to accompany any communication with snch suitable remarks and criticism as, in his judgment, may be appropriate to illustrate principle or explain mistake. His object is the improvement of his profession by the promotion of Science. Simply to propagate *opinion* is not within his views. Already the fair field of Medicine is overshaded by *opinions* that do not instruct, and *cases* too improbable for credulity itself.

This Journal will be published in Quarterly Numbers; each Number containing about two hundred pages. The Subscription to be Five Dollars a-year; or One Dollar and Twenty-five cents for each Number, payable on delivery.

Subscribers residing at a distance, on transmitting the amount of the subscription, (post paid,) to the Editor, shall be punctually furnished with the numbers.

December 2, 1822.

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THOUGHTS

On leaving the University of Maryland, to which I paid a visit, a few days ago, to view the substruction of the Infirmary now erecting.

VIA ET VERITAS.

A PROFESSION, constituted equally of science and art, can only prosper under circumstances suited to the reciprocation of the advantages proper to each department; and nothing can be more gratifying to the cultivators of the important profession of physick than to view under a propitious combination the precept of the school, and the illustration of the hospital; and certainly nothing can conduce more than this mutual co-operation to awaken thought in the learner, or add importance and efficiency to the lessons of the teacher.

In America, as yet, no association of a College for doctrine and an Infirmary for practice, by relative position, internal structure, and general economy, so as to enable them mutually to reciprocate their aid for the promotion of the contemplated objects of each other, evolves the important consequences of a combination and active concurrence of the two. The College teaches; the Infirmary illustrates; but precept can only be general —without practice it evaporates in fumes of speculation; and practice, undetermined by rule, luxuriates in emperical extravagance. The merely scientifick physician

is refined into a pedantick philosopher, while the experimental pretender degenerates to a vulgar mountebank.

The interval between an ordinary Hospital or an Almshouse, visited twice or thrice a week, and an Infirmary intended for the clinical purposes of an University, is immeasurable and not to be traversed by the greatest effort of human reason. But a few months ago the publick patience was trespassed on, and the ordinary terms of civilized usage transcended, by scribblers who wished to instruct without the possession of knowledge, or to obscure interested views by the grossness of calumny. Attempts were made to throw a shade over an University, which, even in its rudiments, has diffused a lustre over our city and our country, and the greatest of its achievements were said to be the ridiculous promotion of private vanity. The most circumscribed knowledge of the subject would have disabused those petty authors of their mistake, possibly imposed delicacy on their language, and added dignity to their sentiments, if it had not inspired some hope of moral improvement or future intellectual qualification to give opinions in matters where the voice of wisdom and experience alone should be heard. But what can avail slanderous insinuation, engendered by unmanly jealousy on barbarous and untamed passion, against steady and persevering virtue, animated by elevated science and patriotick view?

An Infirmary, to co-operate in view and purpose, with a medical school, must be contiguous in site, and so ordered in its economy as that the physicians and surgeons shall daily visit the sick at some determinate period, at which time the pupils may be present. Not

only must the relative situation be such as to accommodate the physicians and surgeons in their proper and necessary intercourse with the sick, but it must be so as to be accessible to the pupils during the hours they are not under lecture, to furnish every opportunity of seeing the course and changes of disease, and effects of the prescriptions—the manner of dressing surgical patients, and of observing the ever varying aspect of wounds and ulcers. Without this, it is not, to the pupils, a source of clinical illustration.

The city-hospital, at the distance of two miles, or the alms-house at that of one mile, and the students to visit the patients once a-week and listen to a course of hebdomadal prelections, would meet the views of an infirmary, subservient to college purposes, as well, but not better, as an hospital in the moon or located among the antipodes, transmitting the definition of its diseases through telescopical nosology. The object of a clinical Infirmary is, to teach personal observation. To listen to what others have observed is not within the scope of practical education. To teach the pupil how to recognise disease under its various diagnostick forms, by personal observation and enquiry, is the peculiar province of clinical medicine. And without the opportunities of observing disease throughout its whole course all hospital education is reduced to mere nugatory discourses addressed by the ingenuity of the teacher to the credulity of the hearer. For whether the symptoms discoursed on, are taken from the nosological definition stored up in the memory or from the patient in the distant and inaccessible hospital, the result to the pupil would be the same. The pupil's object in his attendance on

hospital practice is to learn to observe nature—not to repose and hang on the lips of his teacher.

The clinical Infirmary should in its relative location be such, as upon any surgical emergency, the whole class could be convened in a few minutes; as upon trephining the head, reducing a luxation—laying out a fractured limb and adjusting the fragments—or in the managing a doubtful concussion. The distance even of five or six hundred yards, in rough inclement weather, would present serious inconveniences to both professors and pupils.

The advantages of the Infirmary, as it is now situated, are numerous and great in every way. Together with what has been observed, the whole body of the professors, in different cases, can in a few minutes be called together, and the judgment of those reenforced by the opinions of such of the examiners of the state board as might be within the city: the board of examiners are by a late resolution of the medical faculty of the University, made consulting physicians and surgeons.

Thus, on the first day of November next, I speak on authority, will be concentrated in the University of Maryland every possible advantage, in principle if not in extent, that can be possessed in any University of the world. The class-rooms, already constructed, are superior to any for similar purposes in Europe or America.

Of the abilities and science that preside over and guide the destinies of the college I say nothing; to speak of them is no part of my present purpose. They have already addressed themselves to the world in a

style of their own, and are to be valued according to the prosperity and rapid rise of the institution.

It must be equally pleasing and encouraging to the professors to find their laudable efforts so handsomely and effectually seconded, as they are, by the enlightened portion of the community. In what manner indeed could a polished and enterprising people exercise better judgment or display a more chastened taste than to encourage a profession among the most honorable of human pursuits? A profession which under the judicious disciple of England has given a fair occasion to Mr. Law, of the English bar (lord Ellenborough) to say of it, "I hope nothing I have said will be treated as an illiberal return to my friends for their liberality to my client, for I assure them that I feel as much respect as any man for the learned body they represent, if there be one profession more learned than another, I am willing to yield the palm of that superior learning to the profession of physick."

Whatever nature or the author of nature may have provided in the abundance of its stores or plenitude of his mercies, for the relief of suffering humanity, it must be acknowledged that by disciplining the judgment and copiously supplying the understanding with proper materials, through the labour and study of years amidst the lessons and illustrations of institutions similarly founded to that of the University of Maryland, the physicians of Britain have elevated their profession to that point of dignity and character which justifies the above strong language of Mr. Law, and has called forth from the eloquent Erskine the following courteous tribute of commendation. "Every man knows that for centu-

ries past the physicians of this country have not only been eminently useful to mankind by the administration of that science which is so material for human life and existence, but they have been the most eminently learned persons that have conferred dignity upon society, vastly beyond the bounds of their own particular profession."

If the above few and imperfect thoughts occasioned by the subject, will, in any degree tend to silence the murmurs of querulous discontent—circumscribe the gross insinuations of envious jealousy—rouse to exertion the indolent—or awake to thought the dull, a hard task! they are at your disposal.

LINACRE.

A

PARTICULAR DESCRIPTION OF THE

DISEASE OF SIAM,

OR

YELLOW FEVER;

TRANSLATED FROM THE ORIGINAL FRENCH

OF

M. POUPPÉ DESPORTES,

PHYSICIAN TO THE KING, AND CORRESPONDING MEMBER
OF THE ROYAL ACADEMY OF PARIS.

DISEASE OF SIAM,

OR

YELLOW FEVER.

The disease of Siam, or Yellow Fever, ought to be regarded as a putrid, malignant and pestilential fever. The world have been ignorant, for a long time, of the terrible effects of this disease, in the Islands. It seems that the regularity of its recurrence ought to make us look upon it as one of those maladies, the cause of which is to be sought in the constitution of the atmosphere.

The first event which caused it to be remarked was the stopping at Martinique of a large squadron, which came from Siam, and the crews of which during their stay in this colony were affected by a malignant or pestilential fever which killed a great number of the sailors. This disease attacks very rarely the Creoles or Savage inhabitants of the Island. Europeans destined to live under a more temperate climate, are, to use the expression, its only victims. The extraordinary heat of the colony produces in their bodies changes, from which bodies formed under these climates are exempt, and to which this summer heat is natural. If according to the remark of Sydenham, and agreeably to daily experience, the least change of air is capable of producing fevers, which naturalize as it were, the body in a country, what changes are we not to expect from this extra-

ordinary difference of climate which must produce humours of a density and quality so different.

It is necessary in this disease to distinguish two principal epochs or stages; the first is that of fever, the second is that of metastasis of the disease in which the patient either recovers, or nature, having exerted itself in vain, yields to the violence of the disorder.

DIAGNOSTICK SYMPTOMS.

In the first place, the disease manifests itself sometimes by a chill, but more frequently by great lassitude. The fever which follows is accompanied by violent pains in the head and loins and a heaving in the epigastrick region with vomiting, or a disposition to vomit. The vomiting occurs more frequently when the disease begins with the chill. The fever lasts three or four days without affording any respite to the patient; very seldom does it continue until the fifth. During this period the patients are very much overcome, they have a high and strong pulse, especially in those whose chill has preceded the fever. The skin is often dry and parched; the face and eyes are very much inflamed; the urinous evacuations are sometimes red and charged with sediment, sometimes natural, but in a small quantity, which is a bad omen.

The second stage commences when the fever is ended. However; it ceases suddenly in some, without any other appearance of a crisis than an incipient yellowness; in others, besides the yellowness, a purple eruption, hemorrhage, purging, and vomiting come on. These symptoms are always attended by a pulse almost natural

but generally weak, of a softness or compressibility resembling undulation; with urine very thick and frequently brown.

At this time the patients complain of no pain, and even until the moment of dissolution, appear to enjoy great ease. The majority of those who die of this malady do not outlive the seventh day.

This disease attacks indiscriminately all Europeans who arrive in the colony except some other serious disorder which generally participates in something of the nature of the yellow fever, relieves them from the necessity of paying tribute to it. The more robust the constitutions are, the more have they to dread.

Bilious, and still more, melancholick habits are those which are to be first attacked and which yield first.

Women are less subject to this disease than men: only those who are liable to the chagrin have the misfortune to be attacked by it. The softness of their constitutions (or habits,) and the periodical menstrual evacuations render them less subject to the disease of Siam, and make them recover more easily.

It is remarked that persons of fine complexions and full habits almost all die, whilst those who are delicate are more easily cured. It is necessary not only to observe the constitutions, but also the actual condition of the mind. Those who apply themselves too much to study, to business, or who permit their feelings to operate too strongly, are attacked first and are overcome very quickly.

Of all habits that, which is best calculated to resist the Yellow Fever, is the phlegmatick.

All these remarks will be confirmed by the descriptions of cases which we shall affix to the general history of this disease and its cure.

The yellow fever may be divided into the mild, middling, and malignant, or extreme. The mild is that which terminates in a critical purging on the third, fourth or fifth day, without yellowness, or with an inconsiderable degree of it. When the jaundice is considerable but unaccompanied by any of the unfavourable symptoms, we have described, but which disappears gradually, either by purging produced by nature or the remedies, or by a considerable evacuation of black urine and by the breaking out of several pimples, it may be called the middling. The extreme is that in which no favourable disposition appears at one of the two epochs or crises; the only hope is that nature may produce a considerable deposition or carbuncle upon some exterior part.

PROGNOSTICK SYMPTOMS.

The prognostick symptoms, in this disease, generally, differ according to the period of the malady.

During the fever the chill is a very unfavourable symptom. The patients, who are seized by it, die before the fourth or fifth day, before the cause of the disorder has had time to unfold itself and produce the other symptoms which commonly continue until death.

Even in those who experience only a great sense of weariness if the pulse does not rise but remains soft, one must prognosticate evil; the patients generally die before the fifth day.

But the most sure prognostick depends entirely on the character of the symptoms which precede or accompany the crisis. When there is a considerable discharge of various bilious materials from the bowels or a copious hemorrhage, through whatever part it may shew itself; either through the nostrils, the bowels, or by vomiting, especially if the blood is of a natural colour; when the discharges of urine are abundant though black, where there appears a carbuncle or several blotches, when the patient is attacked by considerable deafness ending in a large swelling of the parotid gland which will disappear in the other crisis, or which will terminate in laudable suppuration, we may generally augur well of the disease. If on the contrary the yellowness appears too early and during the stage of crudity before the end of the fever, if the swellings of the parotid glands and the purple colour appear also before the proper time, (or too soon) if the bowels are always constipated or if they only open to give vent to black materials of the colour of coffee, if there occur also, at an early period, a puking of the same kind, there is room not only to fear for the patient but to dispair.

These occurrences are always attended by great restlessness, a slight pain in the belly, very generally without tightness, sometimes in the upper part and then the hiccough accompanies it; and sometimes in the lower and at length, the suppression of urine, which follows, announces death at hand. It occasionally happens that the whole part is painful. Besides these general symptoms, there are some particular ones equally fatal; in many the orifices caused by the lancet re-open and the blood, in spite of the number of compresses, finds its way

out. This hemorrhage is often accompanied by a carbunculous gangrene which is formed around the orifice, the progress of which cannot be arrested. Some, one or two days before they die, complain of acute pain in some one of the members and particularly in that where the greatest number of orifices have been made : nevertheless this pain attacks the legs and thighs more frequently than the upper part. This pain is sometimes followed by mortification, the suppuration of which, if it can be produced, becomes salutary but very often there appears nothing of it till after death. And no matmatter what remedy is applied it cannot be stopped. This happens commonly to those who have been *too much bled.* They ordinarily have no pain in the belly and remain four or five days in a doubtful state.

Patients complain more of the head, and their bowels are more constipated in dry seasons than in wet. They are also more subject to delirium during the course of the disease. The authrax or carbuncle and dry gangrene are the ordinary crisis in dry seasons, and the opening of the orifices made by the lancet and other hemorrhages in wet.

It consequently appears that the yellowness, swelling of the glands under the ears, and other occurrences are a symptom before the seventh day, and a crisis after this time. If however the fever accompanies them or returns as in the patient in description IX, this fever is then the effect of a critical deposit, from which a too great quantity of matter flows toward the internal parts.

OPENING OF THE BODY.

THE opening of the body presents us a condition which differs according to the stage in which the patient has died, whether during the first days of the fever or at the time of a crisis. Of those who died during the fever, some died at the time of the application of the contagion and before any change is found in the viscera; others have yielded to the violence of the disease, whether it was the effect of their weakness or arose from the virulence, (violence) of the malady. In those last the greater part of the viscera of the lower belly, the liver, the spleen and the small intestines are completely mortified. The spleen is black and soft, and so that the fingers may be easily buried in it, the liver is then black, or of a livid brown, the gall-bladder filled with a black bile of the colour of strong coffee. In those who vomit the black bile, the upper part of the duodenum, the pylorus, and the stomach are partly mortified and partly inflamed; in those who throw it off through the anus all the intestinal canal is in the same manner as the upper part; and in case this matter disgorges itself through one or the other, all the viscera shew the impression of this corrosive matter.

It is remarked in dead bodies that these parts of the intestines where there are turns or hollow places are principally gangrened and that the others appear inflamed. What is surprising is, that patients with similar symptoms have a softness and flaccidity of the whole belly even without pain, at least if not pressed upon with force.

If it happens that some patients have not such evacuations, but die after acute pain in some of the extremeties, the viscera do not appear so rotten nor the bile so black because the poison has reached the part of which the patient has complained. In fact, immediately after death and often some hours before, it becomes purple or black. There is found that in the brain of those who have had violent delirium and lethargick sleep, which is seldom the case, the dura-mater and pia-mater are inflamed and the cortical substance of a reddish colour, which sometimes is communicated to the medullary.

In some who have died on the third or fourth day from the effect of too great plethora the gall bladder is filled with bile partly green, partly brown, the liver with a whitish humour of the colour of cream, the intestines, the stomach, the fat parts, &c. with a matter of similar colour or approaching to it. I have perceived in some, worms; but what is common to all except those who die from swelling, is a corruption so great that in dying and often a long time before dissolution, it is impossible to go near them.

The similarity of the symptoms which characterize the Yellow Fever with those which are found in the aphorisms of Hippocrates, afford room to believe that the inhabitants of Greece and the Archipelago are afflicted by the same scourge and diseases resembling it.

"Quibus in febre morbus regius supervenit ante septimum diem; malum est, nisi confluxus humorum per Alvum fiant."—Aph. 62, Sect. IV.

"Vomitus sinceræ pituitæ vel bilis periculosus; pejorque, si viridis, aut niger."—Cels. lib. 3 chap. 4.

"Morbis quibusvis incipientibus, si bilis atra sursum vel deorsum prodierit, lethale."—Aph 22, Sec. IV.

"Quibuscunque ex morbis acutis aut ex diuturnis; bilis atra vel sanguis niger prodierit postridie moriuntur."—Aph. 23, Sect. IV.

In the course of this disease, three stages are to be observed, that of fever, that of the calm which succeeds it, and that of the termination.

All those who recover from the Yellow Fever are rescued from the arms of death only, when nature produces an abundant purging, a considerable deposite upon some external part, or by a copious discharge of black urine, but the last is very rare. All the indications must consequently tend to assist nature in order to banish and drive out the morbifick matter which produces the disease by some of the crises.

The most common and salutary is purging. It ought to be particularly had in view. The first passages must have peculiar influence upon the crisis which terminates a disease of putrefaction. It is seldom that the seeds of rottenness do not originate in them.

We must also observe that, in the opening of bodies which have died of the Yellow Fever, a greater extent of gangrene is not found in any other part than in the intestines, although the compressibility and flaccidity of these parts, give us no right to attribute it to any inflammatory congestion.

We must then observe general symptoms or indications and strive to diminish the fullness and too great congestion; we must temper and assuage; in a word, to act so that *without* disabling nature we may put her into the condition to sustain the state of weakness and pros-

tration which succeeds the fever, and that sufficient strength be left to enable her to exert herself to expel the matter causing the disease.

With this view we shall proportion the bleeding to the state of body which we meet with in the patients.—We will at the commencement empty them by purgative injection, and afterwards by mollients : early in the disease fomentations and mollient cataplasms will be applied over the whole belly, and care must be taken to keep them warm. They must be exhorted to drink often, and choice must be made of those assuaging drinks which are most agreeable to their taste, because vomiting or a disposition to vomit is a great impediment to the desire of drinking ; and indeed patients under this symptom have little thirst although they appear much heated.—The formulæ of the injections, cataplasms, soups, and barley water which are proper will be found in the collection of remedies at the end of the history of diseases. I prize highly a little whey made with cream of tartar and turned by cresses infused into it; or a decoction of tamarinds a little sweetened, and for want of them weak lemonade with wild orange and a crust of toasted bread to take from it the crudity or raciness.

It is well to mingle this beverage with some cups of infusion of tea or aniseed together, and still better cresses if the patient does not reject them. So soon as the fever subsides a little, it is proper to give up bleeding, and I resolve on purging, which I administer according to the different circumstances under which I perceive the patients, for if they seem disposed to be open in the bowels I only put into the whey some epsom salts or saignette either alone or with some grains of powder of cor-

nachine which I add at the second or third dose according to the effect which the first may have had.

I more commonly use emeticks in porridge, because independant of this manner of purging not being repugnant to the patient, it assists in the intention or object of clearing, if it can be done before the evolution of the bad symptoms, the first passages of the morbifick matter which fills them. However pressing the disposition to putrefaction may be, I do not have recourse to acids of moderate strength in the drink, such as juice of sorrel, lemon juice and spirit of vitriol, only when it is absolutely necessary to calm the vomiting or hemorrhage—because they render the bowels slow and prevent the most general and ordinary crisis. They are after all, generally inefficient, and I prefer a weak infusion of cinnamon in tea, which succeeds much better.

When the patient is at the second stage of his disease, that is to say, when the fever is entirely subsided, we must act according to the different circumstances in which we may find ourselves. The patients commonly seem tranquil and prostrated only during twenty-four hours, sometimes two days, that is to say until the fifth, when signs of dissolution begin to appear. The patient during this time is to use drinks and injections which may suit either to render the bowels more open, if they are not sufficiently so, or to make them so. To their drink some aperient roots are to be added, such as asparagus, dog grass, sorrel and salt of nitre. If too great vomiting fatigues the patient; which is a bad symptom, (for when it has been observed during the fever it ordinarily ceases at this period) some acid will be tried, the juice of the anana, of lemon, carmes water, plasters on the stom-

ach, and the injections will be repeated at least if too great weakness does not render it necessary to suspend them. It is not proper during this period of the disease to administer purgatives which are rather powerful, we should run the risk of throwing the patient into sinking or into weakness under which he might expire. It is only proper, if he is found to possess sufficient strength to season his drinks or tisane with some aperient salt, or to dissolve in them a little manna, if the patient can bear the odour and taste of them. This last purgative is the best and appeared to me to be most successful.

When by the change in the symptoms it is discovered that the bad *principles* have developed themselves and that the blood is infected by them, those remedies are applied which seem suitable to resist them. There are none for the black vomit and black discharge from the bowels, nevertheless for the latter when not accompanied by suppression of urine, for then it is a mortal symptom, weak cordials, according to the strength of the patient, are given, such as confectio alkermes, powder of viper, infusion of cinnamon water. Purgatives are sometimes added to them in order to carry off the offensive materials or matter, which by their remaining can only promote corruption. In too abundant evacuations which weaken the patient too much, I have successful recourse to opium from a third to quarter of a grain repeated. It produces a little sleep, which by restoring the strength, enables the patient to bear the effect of the purgatives which must be repeated. In the perpetual vomiting which continues after the cessation of the fever, to wit, in the commencement of the third stage of the disease, I have met with

success in a bath into which the patient is left, or into which he is put again, according to his strength. The ninth description or case is a proof of this. But if the vomit is black this remedy is no longer in *season*, or seasonable, as appears in the patient of the tenth description or case. These remedies become useless.

So soon as a patient complains of pain in some one of the extremities, it is necessary to apply to it, immediately, fomentations or emollient cataplasms, soothing and maturative, and to wrap up the whole part in order to draw to this point the most of the morbid matter possible, and to procure a deposite there, which is to be opened so soon as there is a little elevation of whatever nature it may be, and the use of cataplasms will be continued. If it is a Carbuncle, it will be scarified and cut across, and digestive remedies applied in order to produce an abundant suppuration, on which depends the patient's safety. The digestives are to be made to stimulate according to circumstances, it is necessary to take care at first, for spirituous remedies are contrary to, or prevent the process of suppuration which it is proper to have. The same thing is to be done to the dry gangrene, of the presence of which we judge by the pains which precede and accompany it. After all, remedies are useless, for I never have seen patients cured who were attacked by this symptom. If there appear some signs of a critical evacuation by the urine, it is necessary to promote it by weak aperient drinks often repeated, some small doses of manna or whey with cream of tartar and cressess. The melancholick, and especially the sanguine bear bleeding better than the bilious and phlegmatick, to whom it must be administered with modera-

tion, particularly the last—better success is to be expected with them from the use of purgatives.

The blood drawn is always very red, vermilion and frothy, containing little of the serous part. If blood is taken after the fever has subsided, it remains for a long time liquid, sometimes three and four hours after the bleeding, and no serum appears on it. In this case, a bleeding repeated is not only dangerous but fatal; therefore it is necessary to prescribe those losses of blood which are thought necessary during the two first days, rarely the third. If however, the fever remains with them, or returns as in the patient of the ninth description, then the fever is the effect of a critical deposition, from which the great abundance flows back towards the internal parts. It is proper in this event to leave the cure to nature, and only to give remedies, in as much as the violence of the fever shall indicate too great a reflux. Mild purgatives and diureticks will then suffice. It is not so in the critical deposites which take place in other diseases. When the fever continues, one ought to follow the signs which the cause of the disease presents, because the question is not, as in yellow fever, to manage and sustain a weak nature which it is necessary always to support in order to assist it in the expulsion of a pestilential poison, the reflux of which towards the internal parts must be avoided.

FIRST DESCRIPTION.

I was attacked by the Yellow Fever the first year of my residence in St. Domingo. I had undergone two

months before, a violent double tertian, and hoped to be exempt, but I was deceived. I attribute it partly to the opening of some dead bodies, in the dissection of which I had been engaged in order to instruct myself in a disease entirely new to me. I had in common with all other patients, great weakness, a prostration of strength and hiccough, which lasted four days—fortunately the yellowness did not take place until the seventh. "Icterus ante septimum lethalis, post septimum salutaris."—Aph. 64, Sec. 4. What I experienced particularly, was a bleeding at the ears which lasted eight or ten days, and which terminated in the rising of a great many pimples or (pustules.)

SECOND DESCRIPTION.

A young man of a lively and sanguine temperament, was seized with great pain in the head and loins, accompanied by constant fever, uneasiness and constipation. On the second day the purging commenced. I was called in on the third. He had been bled three times. I found him very weak with an undulatory pulse, and complained very much of the arm in which he had been bled. One of the orifices had re-opened, and there appeared round it a black gangrene. He went frequently to the easy chair, and passed nothing but black matter. In the evening, the hiccough took place; he died next day. After death, the arm appeared entirely mortified.

THIRD DESCRIPTION.

A man of thirty, of a dry, melancholick, and delicate habit, was attacked by a violent paroxysm which

continued twenty-four hours, and ended in a copious hemorrhage. He became purple all over, without suffering any pain. These appearances continued two or three days, and he got well.

FOURTH DESCRIPTION.

A captain of a ship aged about 40 years, of a very full and sanguine-bilious habit, was seized with lassitude, constipation and considerable vomiting, in which he only threw up water. He was bled four times in the arm, and three times in the foot. The black vomit followed the cessation of the fever; the urine was suppressed, the belly became painful, at length the hiccough was the last fatal symptom.

Bathing might have served to calm and stop the vomiting, and to draw to a point the morbid humours. What prevented my having recourse to it, was the nature of the vomit, which was atrabilious, and of course a sign of speedy death.

FIFTH DESCRIPTION.

A merchant 40 years of age, of a bilious habit, siezed by this disorder, had on the 4th day considerable yellowness, slight pain in the belly, without any appearance of flux. They had bled him twice in the arm and once in the foot. On the fifth day, the first orifice reopened and mortified; purging, vomiting and hiccough succeeded—he died.

SIXTH DESCRIPTION.

A man of from twenty-five to thirty years, of rather a full temperament, sanguineo phlegmatick, and much coloured, was seized with lassitude; pain in the head, loins, and disposition to vomit followed—the eyes were red and the patient very much overcome. He was bled twice on the second day. He fell into a weak state the second bleeding, and they made him take whey tea served with cresses. He evacuated a good deal. In the evening the fever ceased, he fell into weakness several times, and the next day yellowness appeared. From that time the symptoms increased, but the patient went frequently to stool, and the urine appeared black and sufficiently copious. On the fifth day, the first orifice re-opened, the patient became much agitated, covered with purple, had a very bad smell, and possessed very little consciousness, being in a kind of delirium. A grain of laudanum was administered to him in two doses—he became tranquil, and slept five or six hours. When he awoke, the bad symptoms appeared abated. They re-appeared in the evening, but less violent, and continued in the same manner on the seventh day. What gave cause to augur well, was that the bowels were always open, and the black urine passed off copiously. Indeed, from this day the patient got better and better, and aperient tisanes sufficed to cure him.

SEVENTH DESCRIPTION.

A woman thirty-five years of age, of a robust and bilious habit, was attacked a month after her arrival,

with lassitude and slight shivering, a fever accompanied with violent pain in the head and loins. I caused her to be bled twice in the arm, in one day; and the day after, in the foot. On the third, the fever appearing somewhat abated, she took three glasses of cassia water, which made her go seven or eight times to stool. On the fifth, the yellowness commenced—she was seized with disposition to vomit, and slight pain in the belly. I had recourse to frequent injections and emollient broths, which produced discharge from the bowels. The vomiting followed, nevertheless, and continued with violence during two days. The patient passed black matter, both above and below. The face became bloated and she was very much overcome. She did not attempt to take, during these evacuations, any thing but tea, water of cresses, and a little of the confectio alkermes, which she took morning and evening. She was so fortunate as to recover, contrary to my expectations; for during these two days, there was no favourable symptom except the discharge of urine, of which no suppression took place.

EIGHTH DESCRIPTION.

A man of thirty-five, of a dry temperament, was attacked, after a residence of two years at St. Domingo, with yellow fever. He was bled during the three first days, three times in the arm, and once in the foot—they purged him on the fourth—the medicine gave him seven or eight passages. I did not see him until the fifth day. I found him very yellow and tranquil, having only an inclination to vomit, and his bowels a little

slow. He complained after mid-day, of a violent pain throughout the whole thigh and of the leg in which he had been bled, and of so great weight that he could not move it. At first, I only perceived a livid redness about the orifice, which in a short time extended itself very much, rose and became a carbuncle, or black cavity. The emollient and maturative plaster with which I caused the whole leg to be enveloped, contributed to this salutary progress. In the evening it was necessary to open and scarify it, and there came from it a matter, or pus, partly blackish and partly of the consistence of that which comes from large biles, and the blood which come from the scarifications, was blackish. The lower part and middle of the leg were filled with pustules, the sore extended itself and established an abundant suppuration, which cured the patient.

NINTH DESCRIPTION.

I arrived at a dwelling at nine o'clock in the evening to visit a young man of from eighteen to twenty years of age, of a sanguine temperament. He experienced great lassitude and numbness, pains in the head and loins, a disposition to vomit, great heat and warmth. They had done nothing to him the fourth day, but on the second and third they had bled him copiously three times in the arm and twice in the foot. They had administered to him all the sacraments. I found him in agony, the features of his countenance falling, vomiting incessantly whatever they gave him, however without throwing up any black matter, almost without pulse, the extremities weak, the belly painful throughout, passing very little urine ; at the com-

mencement of the yellowness, great agitation and considerable delirium accompanied it. I thought that under such circumstances I could not try any better remedy than the bath and cataplasms; which was done. He could not remain in it the first time more than one or two minutes, weakness in which I supposed him dead caused me to have him taken out immediately. They laid a cataplasm over all the belly. I had him well covered and held by force. Half an hour afterwards the extremities appeared to me less cold and the pulse a little stronger—I caused him to be put again into the bath where he remained five or six minutes. After this operation he seemed less agitated and less tormented with vomiting, he even retained a cup of tea which was given to him. The bath was repeated every three hours until the warmth returned, the pulse restored, and the vomiting ceased, I then had recourse to tisanes made with cresses and nitre; broths, or apozems of lettuce, purslain.

The urine became abundant but a little changed, the yellowness increased and the madness continued. Nevertheless, on the seventh day, having found the belly less hard, less painful, I made the patient take three ounces of manna in three doses with an interval of three hours between each. He evacuated four or five times and fainted twice. The fever which had increased after the baths, continued and became less. On the eighth the fever and delirium were less, but the patient complained of great pain in the right leg. A livid redness was perceived about the orifice to which the same remedies were applied, as to the preceding patient, which caused considerable progress to be made, for all the internal part of the ankle and above the foot fell into mortification and

into so great rottenness that the tendons were naked and the capsules corroded. There was formed beside a deposite at the top of the calf of the leg, which it was necessary to open next day. A severe pain having seized the internal part of the left arm where he had been twice bled, a considerable tumour was formed there, which was opened at the end of twenty-four hours, and from which there issued a large quantity of reddish, blackish, and sanious matter. The copious suppuratives kept up the fever for two days, which degenerated into a slow fever, was stopped by mild purgatives repeated and by the opiate or febrifuge bolus, composed from our pharmacopœia. The foot of the patient remained a little anchylosed. Three things contributed to make this malady so bad. First not having done any thing during the first twenty-four hours ; secondly having carried him by sea to his dwelling; and thirdly having bled him copiously time after time.

TENTH DESCRIPTION.

I was called to an officer of thirty years, of a melancholick and bilious temperament, and native of Provence, who had been three years in the colony where he had experienced a very violent illness ; I was surprised to find him labouring under all of the most violent symptoms of Yellow Fever, because it is rare, when one has undergone a severe illness and become accustomed to the climate of the country to be attacked by it. The complicated double bilious is at most to be dreaded. He was very yellow, very much agitated, and free from fever, the pulse flaccid, a considerable vomiting of black bile fatigued

him. He had been bled twice in the arm and twice in the foot, purged with cassia water and epsom salts, which had cleansed him out well. In the evening the belly was painful, the urine suppressed, he constantly begged that they would relieve him from a great oppression of which he complained ; I had recourse for this purpose to the bath, he stood on his feet in it and remained half an hour without any success ; for an hour after he was seized with sore throat, which was followed by violent agony, which is very rare in this disease.

I might report a greater number of cases which would only differ in some uninteresting symptoms. Those which I have chosen appear to me sufficient to explain the nature of the disease which I have described, and to confirm the prognosticks which we have derived from observation.

REMARKS

ON THE

PRECEDENT ESSAY AND CASES;

BY PÆON.

It is highly probable that more auspicious opportunities, of ascertaining the effects of the use of the lancet in Yellow Fever, have been afforded to but few practical men, than those possessed by the learned Desportes ; and it is equally probable that he used the lancet with as few impressions in its favour or against it as any other physician of any other period or country. There is no reason to admit, from his writings, that he was, either by education or habit, influenced, in any degree as a friend or an enemy to the evacuating plan. And the result of his practice may be viewed as a series of experiments instituted for the purpose of ascertaining the effects of depletion in the management of this formidable malady. At least, if his practice cannot be received as a formal course of experiments, instituted for the specifick purpose, in his cases we may recognize a fair trial of the measures of depletion by the lancet and catharticks.

There is a kind of association, in the mind of almost every professional man, between, inflammation, of whatever nature or sensible form, and the lancet. There, however, is, in this association room for deception and great illusion. By reasoning on loose analogy and by careless induction, the understanding readily passes from phlegmonous inflammation, technically so termed, to

inflammation produced by poison. As the use of the lancet is valuable, and, indeed, for the most part indispensable in phrenitis, gastritis, or any other simple inflammation, it has, too hastily, been inferred that similar beneficial effects would result from its use in inflammations produced by animal or vegetable poisons. Hence, in the earlier periods of our science, it was customary, with practical men, to have recourse to the lancet in small-pox, in measles, in mumps, in bubo from lues venerea, &c. &c. and nothing but a long train of observations and disappointments could have disabused them of their mistake.

The technical definition and sensible phenomena are the same, in general character, in every inflammation. There is, however, a wide difference between the conditions, or pathological circumstances, of the various generick inflammations. That is, there is a fundamental difference in the principles or primary changes upon which the sensible signs depend; but a close resemblance between the general characters of those diagnostick or sensible phenomena.

In a phlegmonous or simple inflammation of the groin, there would be increase of heat, redness, tumefaction, pain, and not, unfrequently, pulsation. The same phenomena exhibit themselves in the venereal bubo. The same general definition or nosological character is expressive of both. There is, notwithstanding this intimate approach in sensible forms, a vast interval between the two inflammations, as to nature and principle. The rabies canina and small-pox are not more distinct. By analogy the practitioner would be led to the same general measures in the removal of the bubo that experience had

sanctioned and proved to be efficient in the cure of the simple phlegmonous affection. And nothing but trial and disappointment would possibly admonish him of his mistake. However, by repeated experiment and observation the more early practisers were instructed in the fact that, let the two affections resemble each other ever so much in form, they are widely different in nature.

In the bubo there is equal disability, to what occurs in the simple inflammation, and a correspondent set of phenomena, but a condition or pathology very different. In the bubo, notwithstanding the measures of evacuation and topical applications, the condition, as to nature continues the same, and the disease in fact remains unaltered and its progress unchecked. If some antidotal agency be not brought into operation, to whatever excess the lancet and purgatives may be carried, the disease remains uncontrolled, and the destruction of the part and general body is inevitable. On the contrary, in the simple inflammation, the mere evacuations will answer our purpose. In this illustration we have a striking example of the facility with which we may be led into errour by pursuing too carelessly the loose dictates of general analogy. In the difference as to nature and essential condition, between the simple opthalmy and that inflammation of the eye that is caused by the application of the matter of gonorrhea, we have an equally important elucidation of our views. The latter, although it may be somewhat checked as to violence and alleviated as to circumstance, by the free use of the lancet or topical applications, cannot be changed as to the nature of its taint nor controlled as to the duration of its action; yet the more simple opthalmy, is for the most part, under the

control of antiphlogistick measures. The iritis when produced by the poison of this venereal, is another example of the inefficacy of evacuations for the removal of inflammations of particular generick characters.

The lessons held out by the above conditions of disease strongly recommend to us caution in our conclusions, and admonish us most seriously in our attempts to resolve all inflammations into an unit, in our practical speculations; they teach us that inflammations, from poison, are not so certainly managed by the point of the lancet.

The attributes of each poison are proper to the particular poison and distinct from the peculiar qualities of every other poison, and are dependent on the intrinsick nature of the poison. Some poisons are determinate, and others indeterminate, as far as our experience extends, as to time; the former go through the round of their action in a certain number of days; while the latter cease only with life; some attack wherever occasion may serve; others but once through life. The danger to be apprehended, from the operation of some, is in the violence of impression during a certain time, and it is the object of art to put the body, during this time, into such a condition as will save its various organs. Other poisons again are only to be overruled by antidotal measures, and if not checked by such, the body is destroyed. Of this class is the poison of the lues venerea; of the action of the poison of the rabies canina, we suspend our opinion, inasmuch as all its cases as yet furnished by faithful history are fatal; whether there be in the stores of nature an antidote, we are not prepared to

say, this must be left to time and the researches of the profession.

Science, leaning on the observations of the present, and guided by the experience of past ages, is positive in the injunctions it holds out, that we do not attempt to abbreviate the time, or cut short the course of the small-pox, or measles, or mumps, or chicken-pox, &c.—— During their progress, its admonitions are that we restrict our efforts to the alleviation as to circumstance ; to restrain the force, and subdue the violence ; but never to attempt to break in on the progress of those diseases. However exhaustion, by the lancet, might change sensible appearances, the laws of the disease would, in principle, remain unaltered, and the lancet could only change a mild into a dangerous, perhaps, fatal malady. Upon subduction of the powers of life, the excitabilities would be augmented, the fever become more intractable, and the grangrenous condition be substituted for that of a more safe gradation. No quantity of blood detracted, however it might lessen the violence and make that safe which otherwise would be dangerous, would arrest the morbid action in the lungs, throat and eyes, in cases of measles ; nor would the removal of any quantity of blood from the system wholly subdue the inflammation and cough proper to the disease. The disease, having once impressed its own laws to a given degree, on the principles of vitality, continues to exert its influence, during a space of time proper to its own nature, and, although the disease is modified as to intensity, provided the body be not too much exhausted, remains unaltered in principle and unbroken in duration.

Yet it were little to our present purpose to demonstrate from the experience and general admission of the professional world, at least the enlightened portion, that the diseases derivable from animal secretions are not to be altered in their laws or cut short in their course, if on the other hand it could be ascertained either from the want of analogy, or from the positive experience of practical men, that feverous afflictions produced by marsh or vegetable poisons, are more manageable by the use of the lancet.

Were the question new, and now for the first time under discussion, we should repose very much on analogy; but, fortunately for the resolution of the point, there is at hand a vast mass of experience, and such as lies within the scope of the observation of almost every man in any degree conversant with practical medicine. And we are extremely gratified that this mass of experience is more or less the common property of every medical man. Proofs, derived from private sources, sources not accessible to publick examination, are uniformly exceptionable, and to be excluded in the discussion of every publick question; the *experience* of every interested man can so easily be moulded to suit his private views and prepossessions.

The Quartan Ague presents, perhaps, the slightest form of fever caused by marsh, or vegetable poison—it excites the slightest disturbance in the animal economy, and is, by consequence, an action more readily subjected to the dominion of any means suited to control the effects of vegetable poison. It is, notwithstanding, a fact of general observation, that this light, and, apparently, insignificant form of disease is not to be over-

ruled, with certainty, by any losses of blood. So far from it, indeed, it is frequently rendered more obstinate by blood-letting. It will, occasionally, continue weeks, and months, and years—the liver and spleen, by the slow, but unbroken action of the morbid laws, will become disordered and enlarged to an excessive degree, and finally the disease will end in dropsy, and the death of the sufferer.

Here the action of the poison is slow and gentle—much time is allowed for the use of the lancet, either during the paroxysms or in the intervals; but yet, where are the evidences of its good effects? Certainly, if in any form of the bilious disease, the lancet could be used as an antidote to interrupt, or supersede the fever, some good effects should be discoverable in the quartan.

At times, it is true, when there is present phlegmasia of the stomach, or lungs, or head, the lancet produces great and salutary consequences, but it should be remembered that this phlegmasia is incidental and not proper to the quartan; and that the lancet, in this instance, only corrects an ordinary phlegmonous action; an action not produced by or dependant on the operation of the poison, but casually superinduced by some exciting cause.—Such as exposure to inclement weather, &c. And the general disease will upon the slightest repetition of this inclement weather, recur and continue to repeat its paroxysms, for an indefinite period, according to its own proper laws, and the resisting energies of the body, notwithstanding any quantities of blood that may be removed. In the slow and chronick inflammation of the liver, or spleen, produced during the quartan by the vegetable poison, the effects of the lancet are very limited

and circumscribed. The body is frequently put, by blood-letting, into a state suitable for the introduction of proper remedies. But here again, the lancet affects rather the intensity, or incidental state, than the *principle* of the morbid action.

The quartan, or any other variety of the intermittent, will frequently be suspended for months, and then recur upon slight exposure of the body—sometimes from the autumnal to the vernal season; and there are instances according to the authority of Bancroft and other good writers, where even the first attack would not take place until six months after the exposure to the poison. During this time, however, it is not to be presumed that the poison remains present in the body. A correct philosophy rather suggests the probability that the vital system has been more or less subjected to the law of the poison, by which, upon the exhaustion of the animal powers, by whatever exciting cause, the disease is brought into operation and evolves itself.

The first accession of the disease after the lapse of six months, or its recurrence after the first attacks, whether at the end of one month or six months, is not to be attributed to any local congestion of the liver, or spleen, or any other organ. On the contrary, such congestion is rather referrible to the presence of the morbid law in the organ affected. And except there be something of the influence of the laws of the poison diffused throughout the living whole, no topical congestion or irritation will renew the paroxysms. This is fairly inferrible from the numerous facts of physconial affections of the liver and spleen, although originally brought on by the marsh poison, continuing for months and years, after

the patient has been wholly free from the general dominion of the poison, without producing an intermittent in any form. Indeed, the feverish state, strictly and properly consequential on those physconial conditions, is rather an hectick, and not an intermittent fever; nor at all of the nature of an intermittent. But to return to the proper subject of our essay.

However inefficient the use of the lancet may be in the lighter varieties of the fever produced by the marsh poison, we are pressed with the *assertion* that the more serious forms of the ordinary remittent and yellow fever, are subject to its control; especially that peculiar irritability and inflammation of the stomach, proper to the Yellow Fever; an inflammation, uniformly *carbunculous* in its character, and derived from the operation of the poison through the common sensory, on the nerves destined to the stomach; we protest against all *immediate* operations of the marsh effluvium on the stomach, as highly improbable, if not impossible.

The question of success by the lancet, in subduing the yellow fever, is to be determined by the general authorities of experience, and not by the assertions of any particular writer. And for this purpose, we have introduced the Essay and Cases of Desportes, and shall fortify them by the general experience of enlightened and ingenuous practisers since the publication of his work.

Dr. Rush, while writing on the introduction and use of the lancet, in his third volume, by some fatality, passes over, in the most profound silence, the name and writings of Desportes, but in his fourth volume, when giving a kind of historical narrative of the different

West-India writers on the subject of yellow-fever, takes occasion to misrepresent the learned Frenchman. It is always decorous, indeed, obligatory, in quoting a writer to give to the reader, the sentiments of the writer, in the words of the writer. They frequently convey a meaning to one understanding very different from that which another finds it convenient to extract from them. On the present occasion, the reader will judge for himself, after having read the following quotation from the 114th page of Dr. Rush's fourth volume, by turning back and comparing it with the fact contained in Desportes' cases, volume the first, and Desportes' rule of blood-letting, volume the second.

"The superior advantages," says Dr. Rush, "of the North American mode of treating the yellow fever, by means of *all* the common antiphlogistick remedies, will appear, from comparing its success, with that of the West India physicians, under all the modes of practice which have been adopted in the Islands. Dr. Desportes lost one half of all the patients he attended in the yellow fever, in one season, in St. Domingo. His remedies were *moderate bleeding and purging*, and the copious use of diluent drinks." We will now let Desportes speak for himself.

"Les frequentes saignées ne sont pas si utiles a Saint Domingue qu'en France; la trop abondante transpiration, et les excès dans l'usage des femmes, en fournissent la raison. Aussi *cinq a' six saignees* suffisent ordinairement." Tom. ii. p. 274.

"Frequent blood-letting is not so useful in St. Domingo as in France; the very copious perspiration, and the excessive indulgence with women is the reason. Thus five or six detractions of blood will ordinarily suffice."

Again—"je pense que si on est appellé le premier jour de la maladie, on doit faire la premiere et même les deux premieres saignées tres-copieuses." p. 277.

"I think that if we are called on the first day of the disease, our first, and even second bleeding, should be very copious."

He also indicates, in the following words, the quantity to be taken. "Des le commencement des maladies dont ils sont attaqués, une livre et demie, et même deux livres de sang."

"At the beginning of the diseases," speaking of the yellow fever and double tertian, "with which they may be attacked, *a pound and a half*, or even *two pounds of of blood*, should be taken." p. 276.

To make four, or five, or six bleedings in the course of two or three days, and take a pound and a half, or two pounds at a bleeding, that is, to detract from ninety-six to one hundred and forty-four ounces of blood in the course of a day or two, or at farthest three days, is not, in our estimation, very *moderate* bleeding——it is, we apprehend, quite equal to any thing accomplished, in the yellow fever, by Dr. Rush, or any other North American physician. Was not the scheme of blood-letting in the yellow fever, adopted by Dr. Rush in 1793, substantially from Desportes? The reader, if he will be at the trouble of looking through the volumes of Desportes, may form his own opinion.

We should not feel ourselves justified in the admission, that any practiser, with or without success, has ever carried the use of the lancet to greater extremes, in the same space of time, than Desportes. In the ninth case, the reader will perceive that, during the second and

third day, the patient was bled five times, copiously; that is, according to Desportes' prescribed rule of two pounds at a time, to the amount of one hundred and twenty ounces in the short space of eight and forty hours. In the tenth case, the patient was bled four times, during one day, and copiously purged. In the fourth case, the patient was bled seven times; that is, to the amount of one hundred and sixty-eight ounces, if the full rule was observed, which, however, is not mentioned. But we are warranted in concluding that the full measure was adhered to from the number of times the lancet was used, and the strong expression already quoted, "that our first and second bleeding should be *very copious.*" p. 277. v. ii.

The question under examination, is not whether, under the circumstances, or under any circumstances, such copious depletion by the lancet, be proper; but whether the course or duration of the fever was interrupted by such immense losses of blood.

We assume, that all the various forms of the fever arising from marsh effluvia, have their own proper laws of action and duration, and that it is the dictate of an enlightened philosophy, never to attempt, after the fever is in regular course, to abridge, or cut it short by the lancet or any other means—the whole endeavour of our art should be to regulate its force, and save, if possible, the individual organs. The result of the copious bleedings of Desportes, tend strongly to confirm us in this opinion.

From the general works of Desportes, we are permitted to conclude, that he lost about half for whom he prescribed; and since his day, experience, as regards

the cases strictly and fairly, under the symptoms proper to that gradation which would fully justify the term yellow fever, has not enabled us to speak a different language without sacrificing truth. It is true that if every feverous sensation, which occurs during the epidemick prevalence of yellow fever, is to be termed yellow fever, the reports of practical men are widely different. But if we restrict ourselves ingenuously, to the rules of Desportes, and consider no case yellow fever except it be marked by the more serious symptoms, such as black vomit, hemmorrhage, petechiæ, carbuncles, or the red, muddy eye, the language of truth cannot go beyond what is recorded by that able man. From 1793, to the present moment, the professional world has had but little reason to congratulate itself on improvement or success with the lancet in this fatal disease.

Entangled in the tissue, and captivated by the specious pretensions of the practical rhapsody published to the world in 1793, physicians have extended the use of the lancet very far in efforts to cut short the ordinary bilious fever of our autumns—such are the effects of a glowing and rich composition without science—and bold conclusion without evidence or argument.

There is nothing more customary, during our autumnal season, than when practical men are called on for professional aid, than to observe, on the first visit, a pound or two of blood is detracted—this is repeated the same, or the following day, until from three to six pounds of blood, or more, shall have been taken away—the patient is reported to be much better; all his pains and aches are gone, and he will be upon his feet in a few days.—But almost as uniformly, within twenty-four, or eight

and forty hours after the last blood letting, the physician called in is sent for—he examines and finds his patient very languid—there is a strong disposition to vomit—the stomach is impatient of whatever it may receive—if the patient attempts to rise, he faints—when left alone, he becomes indifferent to all about him, and is disposed to sleep—he is roused—his mind is unsteady—finally, according to the ignorant and vulgar cant of the times, his case is typhoid. Now for the cordials of all varieties and all sorts; the synapisms and the blisters—and ultimately come into play the yeast and the charcoal, of as much use as so many doses of Album Græcum.

In the first place, for what particular purpose or object are such quantities of blood taken? If there be much general excitement, or local pain, the circulation should be brought down in proportion to the excitement or pain. But why go beyond that? The laws of our intermittent cannot be controlled by blood-letting. The morbid actions in the remittent, are established and sustained by the influence of the same poison, more concentrated, and probably, therefore, more intractable.—Such practice cannot be recommended by successful experience——no fact is more stubborn than that the remittent is at the present moment, under the use of the lancet, more fatal than it was in former years. We ask again, what is the object of the use of the lancet? The poison is not ascertained to act with less violence on a weak, than on a strong habit. But if the body be reduced, and the poison, like every other poison, will exert an influence according to laws proper to itself, it is pretty certain that in proportion as we subduct from the animal powers, we deprive the body of the ability

to resist morbid agency. Not only so, but in proportion to weakness and morbid action, is the stomach rendered incompetent to appropriate that, which, under better circumstances, would be nutrimental; the stomach is filled, not with aliment and beverage, but with solids and liquids, of which it can make no use. And not unfrequently, even after the morbid actions shall have ended, so low is the body, by this sanguineous deprivation, that the vitality remaining is not adequate to the resuscitation of the animal functions; and thus the hand of art performs the work of disease; the patient dies, but not fairly.

By practitioners, in the general, the lancet is used in bilious fever without distinct object or definite purpose; it is used with the vague expectation that upon the loss of blood the fever will stop. But it should be recollected that a fever produced by a poison, is not to be arrested except by some fundamental change in the laws of the morbid action, superinduced by the impression of that poison; the mere raising by stimulation, or lowering by exhaustion does not alter the principle of the action. It is admitted, that the body, by a judicious regulation of its actions may be saved. But in what does this regulation consist? Surely not in mere empirical effects to exhaust. We however once were present when the purpose for which the measures of sanguineous depletion were instituted, was pointed out. It was stated that as the poison contaminated the whole mass of blood, it was proper to remove the contaminated whole, otherwise the spoiled mass would continue to inquinate the new supplies. A suggestion was instantly offered for consideration; it was remarked, if the whole were removed at

once, the heart would cease to beat and a fatal syncope ensue ; and were the circulating mass taken away by degrees, a given quantity at a time, what remained would contaminate the newly added, so that there could never be an end to the disease until finally the whole should be removed, and by consequence the powers of the body would be underminded.

The inefficiency of the lancet is also presumable from a fact which has frequently occurred under our own observation, and is probably familiar with most practical men. During all unhealthy seasons, persons who may be exhausted to any considerable degree by sanguineous losses, are more liable to be affected by the reigning fever than others are. By this it would appear that the relaxed state of the body does by no means disarm the poison of its proper powers, as to the attack ; and if not as to the attack, surely it would not be held philosophick to contend, that it would after its action was already established in the body.

The reader will have perceived by his perusal of the precedent cases, and especially if he has had an opportunity of looking through the whole of the valuable and interesting work of Desportes, that this judicious physician paid great attention to the state of the bowels. He was, however, cautious as regarded the resinous and more drastick purgatives, and restricted his practice to the saline and milder articles ; in the use of these on occasions he indulged with some freedom, yet in general was moderate. His object was to keep the bowels open and liberal. He sometimes, although the patient should have had two or three evacuations by the bowels, during the night, would repeat his catharticks in the morning,

and in some cases, as may be seen at page 250, volume I, the dejections have been provoked to fifteen and eighteen times through the day. His general views were, however, rather in contravention of such frequent and copious evacuation. He conceived that they unnecessarily exhausted the body. We are disposed to believe that his practice would have been improved, as regarded cathartics, had the senna and jalap been occasionally added. Like most French physicians, Desportes recurred frequently to the use of clyster, and certainly with advantage.

Although the use of mercury constituted no part of the measures instituted by Desportes for the management of the Yellow Fever, we may, without impropriety, extend our views to that article, as it is become one on which the hopes of practical men, at the present period very confidently repose.

Mercury as a remedy against the Remittent Fever, in all its forms, and nearly all its gradations, is by some physicians very highly extolled, while by others of equal celebrity, and probably better knowledge, little value is placed on it. It is not, therefore, from the opinions and reports of medical writers and practisers, that it would be proper for us to form our sentiments of an article in such general use, and so well suited, by its medicinal properties, to do much good, or much harm. It is rather from the known and acknowledged effects evolved in the human body, when under its influence, that correct conclusions are to be deduced.

Mercury exerts on the living system an influence by no means equivocal. While under the dominion of its powers the circulation is obviously increased; the pulse is

more rapid, quicker in its stroke, and of greater tension, affording the most incontrovertible evidence of the stimulating properties of this article. The secretions are augmented, and the blood when drawn presents to the eye the sizy covering. Mercury is unquestionably a stimulant, and, by consequence, in the early stage of the bilious fever, is to be used with great circumspection. While the general circulating forces are strong and unbroken, the stimulation arising out of the exhibition of mercury cannot fail to aggravate the condition of any inflamed point or organ. It is therefore prudent to wait until the heart shall have been somewhat subdued, and the powers of circulation tamed by measures of evacuation, and then by a gentle excitation to the arteries of exhalation, the system will be balanced in its actions; the arteries inflamed somewhat roused, and, as by a blister, their diameters in a degree restored.

But here even our hopes are moderated. Whence is the disability of the vessels of the liver, or any other organ, such as the stomach or duodenum? Were this disability, from some phlegmonous action, then would our hopes and our reasoning on anology be well founded, but the inflammation in the yellow fever is carbunculous, is caused by a specifick poison, and in mercury there is no antidotal power, at least as regards the effects of marsh effluvia. Not only is mercury inefficient as to the overruling of the poisonous action, but by acting on a disabled organ may produce effusion, abscess, and destruction of the organ itself; and if not of that, may cause effusion and dropsy in the cavity of the abdomen, or common cellular substance. Hamilton has given to

the world, some valuable remarks on the too early exhibition of mercury.

In the latter stages of pleuritis and hepatitis, we may securely repose on the favourable changes brought about by the careful and judicious exhibition of mercury, as a gentle stimulant in the disabled vessels, thereby enabling the secreting arteries to recover their diameter, and the absorbents to take up the effused fluid and deposite it into the stronger circulating channels. In the yellow fever, however, the disability of the inflamed vessels is the result of the agency of a poison, and so long as the poison exerts its power, or so long as the fever may continue, no article except one possessed of antidotal properties, can possibly recover the vessels, or arrest the inflammatory action. As well might we attempt to overrule the inflammatory action occasioned by the application of the gonorrheal matter to the eye, by mercury.

We do not deny that mercury can be useful as a diaphoretick, and as an exciter of the absorbents, or an evacuant in moderating the disease, in like manner as any other article adequate to the production of similar actions on the skin, the absorbents, the stomach or the bowels. Yet there is much room to question the effects of mercury beyond those sensible physical changes; if mercury extends further, any controlling influence it may exhibit, must be the result of some antidotal power.

Mercury is, almost every day, exhibited in obstinate intermittents; the system is subject to its influence; salivation is excited; but still the quartan or tertian paroxysms recur. They are, it is true, sometimes stopped while the body is under the influence of mercury.

But here we presume the mercury merely relieves the organ from a state of congestion, and thereby the animal powers are enabled to recover; or the very circumstance of keeping the body defended from the effects of inclement weather, night-air, or debauch, may be the indirect mean of restoration, the mercury having no immediate agency in the affair. We are strongly fortified in this latter suggestion, from the fact, that not unfrequently, notwithstanding the most regular use of mercury, these paroxysms of the intermittent continue to recur; even during salivation, and sometimes with augmented violence.

Were the mercury possessed of antidotal powers, this could not occur. The action of the mercury being diffused and universal, no new morbid action, on the ground of antidotal agency, could possibly take place; the impression of the antidote would prevent or remove the action of the disease. This is the fact in the lues venerea, and must of necessity be the result in every other disease, if there be any other disease, in which mercury exerts antidotal powers.

During the course of the common bilious, or yellow fever, it is frequently observed that, at the accession of exacerbation, the mercurial action of the mouth ceases, and, immediately on the decline of the exacerbation, recurs again. This fact is remarked by almost every practical writer, who has carefully detailed the phenomena of the rise and decline of remittent exacerbations, during the full and sensible operation of mercury.

We have also had opportunities of observing, under a full charge of mercury, for the venereal disease, the

attacks of yellow fever, and the patients to sink under them. In such instances the strongest evidences are furnished that, in mercury, we are to expect nothing like antidotal powers. It may also be subjoined, which is observed almost daily during the prevalence of our autumnal epidemick, that, when the mercury is urged in the severe cases of fever, the gums, insides of the cheeks and fauces, will fall into gangrene and mortification, from the excessive action of the mercury, but yet the fever is unchanged and unabated.

It is in the mouth of every friend to the mysterious operation of mercury, that if a regular salivation be excited and kept up, the patient recovers. This is true; but why not add, that it is only in the decline of the fever, or in the milder forms of fever, this salivation can be excited or kept up. The salivation should rather be considered as an evidence than a cause of the decline of the disease. We must conclude that mercury is but a mean in the hands of the physician, and like any other medicinal article, may be useful or otherwise, as good sense or folly may wield it.

The object of our present remarks, is, to fix the attention of the reader on the important point, that the use of the lancet, of catharticks, and of mercury in the bilious or yellow fever, is to be regulated by discretion; —by a careful examination of the condition of the body, and observation of the phenomena present, as is proper in the use of these or other means in the management of other diseases. We wish particularly to recommend, by what we have said on the use of the lancet, to the serious consideration of the reader, the great and important necessity of always bearing in mind, that, however

useful and valuable, the lancet may be in phlegmonous inflammation, the same good effects are not to be expected from it in the carbunculous, or if the reader prefers it, the erysipelatous affections of the stomach, or duodenum, in the yellow fever. Wherever there may be any thing of simple or phlegnomous inflammation, whether in the brain, the lungs, or the liver, or any other part, our dependence is on the lancet—but wherever the inflammatory action depends on the poison, the lancet, having no antidotal power, will in a great degree disappoint our expectations. As has already been suggested, it would be as well to attempt to cure a bubo, or to regulate the various eruptions, or the petichiæ, or a carbuncle on the surface of the body, as the peculiar internal inflammation in the yellow fever, by the lancet.

We have, sometimes, listened with indescribable astonishment at the earnest and positive recommendation of the lancet in the yellow fever, because upon dissection, after death, the inner surface of the stomach—the duodenum, or gall-bladder, was found affected by an inflammation resembling erysipelas! Why not the same earnestness and warmth in the recommendation of simple sanguineous depletion in bubo? It would be equally effectual; or rather, equally useless, but not quite so destructive.

We most gravely and respectfully present our views to the reader, and entreat him to look well into the possible changes that may be brought about by an unadvised and empirical indulgence in the use of any measure—that he endeavour to ascertain some definite object or end that is to be brought about; and that he weigh well the result of the trial of the lancet and mercury, used

as they have been, for the last twenty years—and that he endeavour to solve the question—why is it that both the ordinary bilious and yellow fever are more and more unmanageable every year? And also the equally important question—why is it that our winters are so fatal in the districts where the bilious fever prevails in the autumn? Is this increase of fatality owing to the very exhausted state of the body, brought on by the repeated and improvident, and almost systematick use of the lancet? the increased susceptibility of the body after excessive salivations and indulgence in the use of mercury? or to those two causes combined? There must be some adequate cause, and we can scarcely persuade ourselves that, under fair management, the liver would remain so diseased from the alone action of the poison during the summer and autumn, as to be the sole cause of this extensive fatality.

The experience and observation of more than twenty years have not ascertained the superior advantages of mercury in the treatment of the intermittent, the bilious, or yellow fever. It may be contended that by the use of mercury, the obstructions of the various viscera will be removed. This is granted, but are those obstructions the cause of the origin, or the continuance of the fever? The intermittent, the remittent, or the yellow fever, will have origin, will continue their course, and will destroy life without visceral obstruction; and visceral obstruction will take place, continue for weeks and for months, without producing either Intermittent, Remittent, or Yellow Fever. Both the obstruction and the fever are produced originally by the same poison, and the presence of the one may possibly aggravate the

other; but it is well ascertained that the cessation of the fever is not, as a consequence, followed by a subsidence of the local tumefaction—nor is the removal of the local affection, succeeded by a cessation of the fever, as a consequence. Although they may co-exist or follow in succession, they are not dependent on each other as cause and effect.

Therefore, although by the use of mercury, the congestion might be removed, which is very problematical and uncertain, as the fever is not produced by, or dependent on, the infarction, the fever would still continue. It is admitted, that if the pain and tension of the part be lessened, the fever is diminished. But this is rather ascribed to the circumstance, that the same measure which lessens the local, equally diminishes the general disease—and that which diminishes the general, equally lessens the particular affection. Physicians who speak of diminishing the general fever by removing the particular obstruction, surely mistake a particular for a general action. As both the particular organick affection and general fever, are attributable to the operation of the poison, so the diminution of the one or the other, or both, must be ascribed to some particular and general action of the therapeutick measure. That by removing the obstruction in the liver, or any other organ, the fever is commanded, is a puerile conceit. It is true that a diminution of fever frequently succeeds to a removal of obstruction in a particular organ; but it does not take place in consequence of the removal of the organick obstruction; it occurs, because there is a change in the whole animal system.

Then what have we gained by the experience of twenty or thirty years in the use of sanguineous deple-

tion and salivation in the treatment of the intermittent, or the common remittent, or the yellow fever? We have gained a thorough conviction that the human family has come by great loss of comfort and of life from the charlatanical manner in which those measures of blood-letting and salivation have been indulged in by those who are too ignorant to think, or too idle to be educated. We have gained the important conviction that a correct science neither wholly rejects measures of depletion on the one hand, or abandons cordials or stimulants on the other; that it appeals to the one or the other as circumstances may indicate—or employs them in succession, admitting the old Latin maxim, that, "stulti soli dum vitant alia, in contraria currunt."

It has, no doubt, excited the surprise of the reader, that we have passed over the opinion of Desportes, expressed in the text of his essay, and also in the first of his cases, in respect to contagion, especially as Desportes declares the yellow fever to be a disease, the cause of which is to be sought for in a peculiar condition of the atmosphere. Our silence is not casual, but as the subject has, by the able pen of Bancroft, been so thoroughly exhausted, we could not prevail on ourselves to take it up again after it had passed through the hands of so competent a writer. As regards priority of claim, however, in the opinion that no disease, derived from marsh poison, can, under any circumstances become contagious, the reader is referred to a pamphlet published by the editor of this Journal, in 1798. Notwithstanding what had been written by Desportes and others on this interesting subject, we are persuaded that the work alluded to, is the first in which any thing

definite and decided on the general principle of marsh diseases being altogether incommunicable by an intercourse of persons, is to be found.

CRITICISMS

ON THE

EMPLOYMENT OF MERCURY IN FEVER.

BY EZRA GILLINGHAM, M. D.

N. B. The following essay was written a considerable time since, and before the opposition to the mercurial treatment of disease had become so decided as it is at this period.

To understand and interpret the morbid condition of the system is an office of great difficulty, yet to remove a large portion of its diseases is ordinarily an extremely easy and simple task. That the disarrangement of an exquisite machine should not require a skilful hand to restore it to order, would appear to be an absurdity, if indeed facts had not long since demonstrated its truth. The same power of mind which would forever be defied by an irregular watch, finds no difficulty in regulating the human system. The explanation of this is to be found in the simplicity of its diseases, and in the wonderful protection which its life affords against hostile agents, whether purely injurious or intended to be medicinal. It is indeed no easy matter to destroy life by an erroneous medical practice, and when the frame is diseased, it very often requires only a purgative or emetick to remove all that impedes the natural operations, and to restore health.*

* Dr. Huxham sarcastically advises those who, as he expresses it, neither "read nor reason," to peruse the sixth commandment.—

There is an immeasurable distance, however, between the qualifications which enable a man to combat successfully those simple cases of disease, for the treatment of which private gentlemen are very often sufficiently adroit, and those which prepare him to meet difficulties by which the mightiest intellects are confounded. But there appears to be an agreement among many of the profession, and some of these, too, men of superior light, that this almost self-evident proposition is entirely erroneous. Such, at least, is the inference which we are allowed to make from their conduct; for it is a practice, in the defence of which numbers of the learned and the ignorant have been almost equally arranged, to attempt the cure of a fever through all its various stages by the application of a single remedy. This remedy is mercury. Its employment prevents the necessity of intellect, of enquiry, of laborious study, of powerful and anxious vigilance; but it is the opinion of many that this Sampson, as it is called, instead of destroying that against which it is opposed, more often, to use the language of one who was the warm advocate of the mercurial practice, "pulls down the pillars of the human constitution."

The approach of mercury towards that astonishing and extravagant station which it holds among the remedies for fever, has been very gradual. Physicians dreaded its power and adopted it with caution. I do not mean to deny its usefulness. On the contrary, I acknowledge it to be of unquestionable value, and be-

These, however, cure or suppose that they cure so many, from the causes which I have mentioned, that his advice, if followed, I am afraid would be productive of no good result.

lieve that its true reputation can only be destroyed by the fondness of its friends.

The errors in the practice with mercury in bilious fevers, may be arranged under the following heads: First: It is given at every period of the disease. "The general testimony of practitioners," says Dr. Warren of Boston, "who have been much in the use of this medicine, is in favour of its exhibition in all stages of febrile disease, even in the very commencement of the fever;" and as if he wished that there should be no misunderstanding, he rather absurdly adds, "*or before it is formed.*" Second: It is employed in greatly excited states of the heart and arteries. Third: If there should be local disease accompanying the fever, it is given in all its stages and sometimes without regard to the propriety of previous evacuations. Fourth: It is administered in immoderate quantities. Fifth: It is used by many in order that it may exert a specifick stimulant power independent of its known properties. Sixth: It is employed to destroy feverous action by its own, or the mercurial action.

I. Every rational system of practice must have a regard to all the alterations in the condition of the animal œconomy. Prescriptions are to be changed as the morbid states vary, and to be made, if possible, to answer to clear and precise indications. Let us apply this rule to the customary administration of mercury.

Our attention is always called forcibly to the alimentary canal in the highest form of the bilious fever in our country. We know it to be the principal door by which we obtain an entrance into the system, and we find it absolutely necessary during the first stage of the

disease, to remove whatever clogs or obstructs it. It is a serious matter to select the best means of performing this office. The older writers do not appear to have considered it very important, but those who acknowledged the value of purging, generally prescribed simple medicines. Hillary, in particular, directs only manna and tamarinds, which, he says, seldom or never failed to produce the effect intended. Hillary page 172.* My own experience leads me to believe, that much more might be accomplished by the aid of simple purgatives during severe fevers, than physicians are generally willing to grant. However, we will suppose that it is extremely difficult to move the intestinal canal, which although it is very frequently, is far from being generally the case.

Practitioners who use mercury with freedom, commence with large doses of this medicine, and no evacution is produced. This result is to be referred, in part, to the quality of the mineral, which seldom operates with facility in large quantities, and in part to the debility of the intestinal canal, being increased by the stimulus of the mercury. The dose is repeated again and again, and many hours pass before the object is accomplished, if it be at all effected. In some instances, that which probably might have been obtained with facility by some of the more simple yet more active evacuants, cannot be at all produced, because the stomach and bowels are rendered insusceptible of their power. In the mean time the disease is rapidly progressing, and

* Jackson disapproves strongly of the practice of Hillary. It is not my intention to establish its propriety. My object in introducing his authority is merely to give evidence of the power of mild purgatives over the alimentary canal.

our power over it is proportionally diminishing, for that which is to be done for the relief of this malady, must in general be done during its earliest period.

Here, then, is a decided testimony against the custom of prescribing without paying regard to the condition of of the system. This employment of mercury is evidently empirical.

Even if we designed to use mercury with a view to its general influence, it would, in most instances, be better to empty the intestines previously. Its effects would always be more decided and less injurious. Its operation as a stimulant could then seldom be so pernicious, as it is in the manner which it is now used, nor would there be so much likelihood of its increasing congestion in the viscera.

Other remarks on this subject will be found under the remaining divisions.

II. Physicians appear to view mercury as though it were possessed of a kind of Protean power, by which it becomes accommodated to all the needs of the system. Those, therefore, who regard it as the most useful of our purgatives, often prescribe it in very excited states, with the expectation that it will subdue rather than increase the power of the heart. Others, however, who perhaps place less confidence in mysterious operations, think that they observe it to exert, under such circumstances, its usual stimulant properties. Of the truth of the latter observation, there can be no doubt, as regards bilious fever, in any of its forms.

I believe that it will accord with the observation of most of those who have adopted both measures, that the use of the pulv. jalapæ comp. or of some of the

salts combined with a small portion of tart. antim. will accomplish the object of subduing the force of the heart in fever, much more effectually than any of the mercurial preparations.

If mercury should be administered because of some morbid affection of the liver accompanying the fever, it cannot operate favourably if there be violence in the general action—if it should be given during an excited state of the circulation, when hepatick disease is not present, it may produce it by increasing the general disturbance. Fevers in which the action is vehement require prompt and early evacuation of the stomach and bowels. Mercury is in general an unsuitable agent because of its slow operation, and the certainty which we have, that its influence will be injurious if it should not operate. By confidence in it, and of course abstinence from other medicines in expectation of its operation, we shall lose many important moments, and by its stimulant power we shall increase the feverous action.

III. In the inflammation of the liver, we are taught by the best writers, and by general experience, that mercury is not to be resorted to as the first means of cure. It will not be useful to prove this by many quotations. I have selected one from the most popular treatise of the day, on hepatick affections. "In order," says the writer, "to successfully combat this disease, we are first to check the general excitement of the system and the local inflammation of the organ as much as possible by copious vensection at the arm, &c.* Mer-

* Johnson, on Derangements of the Liver. An apology might be necessary for this quotation, were it not known how soon physicians

cury is undoubtedly a stimulant, and therefore the appropriate period for its usefulness, must follow the reduction of the forcible action of the arteries and heart. Physicians are so sensible of danger from the employment of this remedy, that they all unite in discarding the thought of salivation in hepatitis without previous evacuation. Many however, and Johnson among the number, esteem it a proper purgative in the first stage; but, I think, from the want of sufficient reflection, for if it be ever injurious because other remedies have not preceded it, its purgative operation cannot entirely deprive it of its noxious influences.

If all this caution is so important in the treatment of simple inflammation of the liver, why should it not be so likewise in that by which fever is made so complex, and so difficult of management? If inflammation should exist during fever, does the latter change the nature of the former? Or will not the local disease require the same precautions, and the same vigilance and care?

The minds of physicians appear to be much governed in their choice of mercury in bilious fever, by the fear of a latent existence of inflammation, or inflammatory congestion where the signs do not appear, or are not distinct. It would become an enlightened mind, to examine very carefully into the evidences of this complication before its relief should be attempted. "De non existentibus et non apparentibus eadem est ratio." To

commence the use of mercury in this affection. An inflammatory disease of the muscular system, was, indeed, a short time since, treated with mercury in doses of 10 grains every three hours, without any previous evacuation, because, unfortunately, the physician had heard that pain in the side, was a symptom of hepatitis.

prescribe for the relief of what we cannot discover, may be often safe, but it also may be often destructive, and it certainly is not rational.*

I have often been convinced that physicians make supposed discoveries of hepatick disease when the liver is entirely free from any other disorder, than what every organ must more or less suffer during a morbid condition of the general system. A physician will approach a patient and press upon the region of the liver with his hand, a thickness sometimes of several folds of clothing being interposed, and if there be any tenderness, it is very often considered sufficient evidence of hepatick inflammation. This tenderness may arise from a variety of sources. The muscles, the peritoneum, the bowels, the stomach may all be extremely impatient of pressure when it is applied over the hepatick region, while there is no serious disease. Those who practice in bilious fever, often observe very great tenderness and some pain to affect the neighbourhood referred to, which will disappear entirely in a few hours. The skin is sometimes sore in fever throughout all its surface.

Some examples may place in a stronger light, the possibility of deception in judging the signs of hepatick disease unless very great caution be exercised. A man labouring under typhus fever had an extreme tenderness under the edge of the ribs of the right side,

* Perhaps I should speak with caution on this subject. I confess myself a nosologist, and as a great many advantages are supposed to be derived by an ignorance of nosological science, it is possible that the art of prescribing where there are no signs to direct us may be one of the number.

accompanied with a severe pain in the right shoulder. During the existence of these symptoms, an abscess broke and discharged a large quantity of purulent matter through the thrachea. As the tenderness in the hepatick region had been so acute as to lead to the suspicion that an abscess was about to open on the side, there appeared to be no reason to doubt that it had taken another course and passed from the liver into the lungs. After death I examined the body, and discovered no effect of disease whatever to exist in the liver. It appeared to be of the usual size and perfectly free from morbid appearances, although I separated it from its connexion with the diaphragm and made deep incisions into its substance. The abscess was in the lungs and the right side of the colick arch appeared very much inflamed.

I do not remember a more remarkable example of the possibility of deception in this particular, than the case of a woman in this city. Her disease was in the uterus, but symptoms arose which apparently marked an inflammation in the liver so strongly, that a salivation was produced. Relief succeeded for a time, but a tender state of the womb recurred, and the tenderness spread over the peritoneum so as completely to simulate Hepatitis in almost every one of its signs. The pain in the shoulder was particularly expressed. Every symptom was, however, relieved by one enema of starch and laudanum, and many months passed away without any return.

To speak of relieving hepatick disease without the aid of mercury would appear exceedingly preposterous to many persons. But I know by experience that neither in simple hepatitis, nor in its complication with

fever, is mercury uniformly indispensable. There is nothing in the nature of this combination which forbids us to adopt the same means which would be useful in inflammation of some one of the neighbouring viscera.*

It is often ludicrous to be made acquainted with the operation which many physicians expect calomel to perform on the liver. Thus Dr. Rush and Dr. Griffitts appear to have exhibited it "in order," to use the words of the former, "to loosen the bile from its close connexion with the gall bladder and duodenum."

IV. As it is somewhat questionable whether or not those who employ very large doses of calomel can in general give any very rational explanation of their object, it will not be very important to oppose them. If there be any propriety in administering doses of 30, 40, 50, 60 or more grains of calomel, I acknowledge my ignorance of what it is which establishes that propriety. It is probable, however, that their reasoning is arithmetical, and that the proposition may stand thus: if a dose of 10 grains of calomel produce a benefit to the patient equal to 100, what benefit will a dose of 60 grains pro-

*It had been a matter of a little surprize to me, sometimes, that liver disease should be so prevalent as to be almost epidemick—I mean, concluding from the reports of physicians. I have heard of some, of whose patients such an overwhelming proportion afforded signs of hepatick disorder, that I could not avoid recollecting a scene in Moliere, in which the Malade Imaginaire is told that the symptoms most foreign from pulmonary disease, and even that the signs of health, are evidences of the disease of the lungs. It would be often to the advantage of society, if patients had sufficient judgment to discover how unimportant a little knowledge is without sagacity.

duce.* Immoderate quantities have been given during the continuauce of a fever, and he who was hardy enough to give the most, has often been considered the greatest physician. Dr. Porter gave in Tortola $2\frac{1}{2}$ ounces and 2 drams of calomel; some one more adventurous exhibited 4 ounces and 80 grains.

V. The doctrine that mercury cures fever independently of any of its sensible operations, was sustained by Dr. Rush. He speaks of it as a counter stimulant to the miasma which produced the fever. He supposes the stimulus from the remote cause to be equal to five and of mercury to three; by bleeding, he says, the febrile action must be reduced to two and an half, that the stimulus of the mercury may transcend it. Transcend it? Transcend a state of febrile action in order to produce health!

We may discover that Dr. Rush had no distinct notions of what this transcendency meant, by contrasting this strange doctrine with another part of his works. He observes that where there is an excess of the stimulus of the remote cause, the mercury meets with no resistance in its tendency to the mouth. "Bleeding," he continues, "in this case would probably do harm, by taking off a part of the pressure of the system, and thereby produce a reaction in the vessels, that might predominate over the action of the mercury.†" Such language has often been fashionable since the days of Dr. Rush, but there is a difficulty in understanding it. The

* Some heroick warriors against fever administer it by teaspoonsful.

† "The disease does that for us by its force, which in other cases we effect by depleting remedies."

translation of it appears to be this ; that if the remote cause be of a certain degree of force, mercury is incapable of curing its effects, but the more severe the disease, the more easy the relief. This fact is extremely doubtful.

It is common to talk about equalizing the excitement and excitability, or making them proportionate, and many a person has spoken wisely about creating a balance between them. This is certainly absurd, for as excitability is the capacity of the body to be acted on, the action must always be in proportion to the incapacity of resistance ; there cannot be a disproportion.*

While physicians have no clear notions of the nature of this peculiar and specifick operation of mercury, it seems important that caution should at least be inculcuted in relation to its exhibition. If we know nothing about the mode of the operation of an agent, we should at least be certain of its beneficial effects.

I am satisfied while this operation of mercury remains mysterious and equivocal, to avoid making use of the remedy in a febrile disease, except with some reference to its known qualities. They are undoubtedly of very great importance. It is purgative, emetick, diaphoretick, affects the secretory process in various parts, operates upon the absorbent system, and creates a peculiar action in the body, which is particularly indicated by an increased flow of the salivary fluid.

VI. I come now to the opinion that mercury is sanative in Bilious Fevers, by the substitution of that pe-

* This simple and familiar explanation of an unnecessarily difficult term, belongs to Dr. Davidge of this city.

culiar action which is manifested by a ptyalism. It was a favourite principle with Hunter, that no two actions can subsist in the system at the same time. The extension of this principle has been one of the chief causes of the present mode of practice in Bilious Fevers. It is common in severe fevers to make every effort to charge the body with mercury ; the intention being to make the mercurial predominate over the febrile action. If no success should be obtained in establishing the former, the disease is considered incurable. It is however the opinion of many judicious physicians, and among the rest of Dr. Jackson, that a salivation is in most instances only an evidence that the fever is terminating, and that the system has recovered enough of its life to receive the mercurial impression. I say in most instances, because, although it was once thought that the fever uniformly terminated upon the establishment of pytalism, yet now we know that this was a mistake.

Dr. Rush acknowledges that mercury seldom salivated in 1794 until the fever had intermitted or declined, and that in many instances there was no salivation until the morbid action had altogether ceased. It is evident that the mercurial action could not have been curative, for the signs of it did not appear until the necessity for it had ceased. It was not established during the earlier part of the disorder, becanse the nerves of the stomach and intestines had not a sufficient quantity of vital power to feel and acknowledge the impression. When the force of the impression had begun to appear, the system had begun to recover its healthy state.

In almost all-cases of fever, which are not attended with sufficient local disease to require unequivocally the

agency of mercury, it is useless to employ it with a view to its salivant action. We cannot, indeed, produce a ptyalism in general, until such a state is wrought by other means, so near to that of convalescence as to render mercury unnecessary. During that excessive prostration of the vital power, which occurs in Yellow Fever, mercury is generally used in vain, with reference to any constitutional operation. The vehement exertion of the heart and arteries, in whatever variety of Bilious Fever it may occur, is also an unconquerable obstacle to the proper effects of mercury. After we subdue this, the system, if its disease be curable, can commonly be restored to health without any mercurial influence. There will be no necessity for the new action; it will, when excited, be only the addition of a distressing and painful malady to an exhausted and worried body. It will be a barrier to convalescence, by forbidding the use of food which might otherwise have been proper, and will indeed be often a worse malady than the fever itself.

It is however useless to reason against this mode of employment, for the supposed fact, upon which it has been founded, is entirely erroneous. The mercurial action is not incompatible with fever. The latter often continues after the former has been established, and terminates in death.

It is by a reference to experience that we should determine any controversy concerning practical subjects. It is a melancholy truth, that this most infallible of all human teachers has shown, that mercury indeed deserves the epithet which its devotees have delighted to give it, a Sampson, powerful, terrible destructive. I have examined the testimony furnished to us from the

West Indies, and find no evidence of advantage having been gained by the use of Mercury in the Yellow Fever. In the southern extremity of this country, its effects were dreadful to a part of the British army during the late war. I have, however, concluded to draw my testimony from our own city.

During the fever of 1819, there was, fortunately, an admirable test furnished of the success of the treatment. From the 1st of the 9th month, to the 18th of the 10th month, daily reports were made to the Board of Health by all the physicians of the city of every case which had fallen under their care. The whole amount of cases reported as malignant during this period was 859; the number of deaths 313, forming a proportion of 1 death out of every 2.74 persons. It appears, however, in the annual reports of burials during that year, that 350 died of malignant, and 73 of common Bilious Fever. As some of the physicians who reported these cases, were of the opinion that every fever which occurred within the infected district was necessarily a malignant fever, and others were strongly suspected of being willing to report very mild, as malignant disorders, it is a fair supposition that 73, of the 859, were not more malignant, than the 73 who died of common Bilious Fever. This would leave a remainder of 786. The fever commenced about the 14th of the 8th month, but it appears from the books of the Board of Health, that the cases were not numerous. Notwithstanding there were 37 persons who died of this fever, either before 1st of the 9th month, or after the 18th of the 10th month, who are not referred to in the above calculation, the proportion would still be clearly established. For as 313 bears

the same proportion to 37, as 859 bears to 102, it would require more than the last number to have been ill, previously to the 1st of the 9th month, to invalidate it, which is probably 60 more than were sick before that date. A large part of the number 37, should belong to the number 313. A considerable portion of them died after the 18th of the 10th month. Allowing then the number of cases to have been 786, and the deaths to have been 313, the proportion of deaths would have been 1 out of every $2\frac{1}{2}$ nearly.

Mercury during the fever of 1819, was used to its full extent, and here is its boasted triumph. This fever was far more fatal than those which were formerly treated on the tonick plan, although this was also erroneous.

A dogma has long since been established, that, as it is commonly expressed, "Herculean diseases require Herculean remedies." This has led more to an extravagant use of mercury, than perhaps any thing which has ever been written. It seems to be in direct opposition to the elder maxim, contraria contrariis medentur, and is much less true. One may be at liberty to conclude that at least in some instances, one Hercules is quite sufficient to demolish the system without the aid of the other.

"EXCHANGE OF FUNCTION,"

EXAMINED BY PÆON.

DARE VERBA ALICUI.

We have read somewhere; in some book, or fraction of a book, that "the uterus and mammæ often exchange function;" that is, engage in alternateness of action.

For two organs mutually to perform offices for each other, it is necessary that each, in its turn, go through the function for the other. If the breasts and the womb reciprocate functions, not only must the breasts secrete, according to the present acceptation of the term *secrete* among physiologists, the menstruous fluid, but receive the conception from the ovaries, bear it nine months, nourish it during that period, and eventually, deliver it into light. And not only so, but the womb, on its part, must make the lacteous aliment for the child, after it is born, and provide the structure through which this alimonious fluid is to be conveyed into the child's mouth. The sentence "exchange of function," is otherwise without its import, and becomes a mere chain of words, insignificant and without meaning.

To exchange function, is an expression that bears relation to the whole, and not a part of an office—reciprocation implies a full return. This is an axiom in logick, about which even school boys could not dispute. As a corollary, then, it follows, and that

necessarily, that the breasts do, at times, perform all the functions of the uterus, and that the latter organ does make a full return; that is, performs all the offices of the breasts! !

This, if it be a fact in the natural economy of the body, has hitherto remained hid among the unexplained realities of nature—and if it be a mere speculation, it is, to our understanding, more curious than probable, better suited to provoke a smile than enforce conviction, or enlarge the field of science.

Although the subject is presented to us in a new light, yet this reciprocity of function has not, altogether, remained without the scope of our consideration; but hitherto our decision has been in the negative. We have been led to believe that no organ or part of the animal structure, could perform the function of another organ, or part.

Every organ, or part of the animal body, is possessed of laws of life, and by consequence, a structure, proper to itself, and dissimilar to the laws of life and structure of every other organ, or part. There otherwise could not be distinct organs or parts.

That the uterus or maternal appendage secretes a fluid, suited to the tender embryon, it bears, is an opinion offered for physiological examination, first by yourself in 1793, and, as we are told, by professor Jeffray, of Glasgow—or as professor Monro had previously suggested, the absorbents of the placenta acted the part of the lacteals, and selected from the mother's blood that which was appropriate and nutrimental. But it does not appear that, in the opinion, first offered, there was any thing lacteal supposed to be in the fluid

furnished by the uterus, if, indeed, such an opinion is to be tolerated, as that the uterus secretes any thing, except the menstruous fluid. We have remarked that if a fluid be absorbed from the mother's blood, the act of absorption is performed by the proper instruments of absorption, and not by the veins, as has by some been admitted. It is in our recollection that, three or four years ago, in discoursing on the subject of absorption, in your place, as professor, you strongly insisted on the probability of the absorbing vessels, in the various cavities of the body, in many instances, ending in neighbouring venous trunks; that, in your view, the two trunks terminating in the subclavian veins, were inadequate to the general purposes of absorption. And you urged it on the class, that this opinion was strongly fortified by the analogy of the horse, in which the absorbents, according to the experiments of some anatomists, in several parts of the body, had been traced to, and found to empty their contents into other veins than what, in the horse, may answer to the human subclavians. But to return to our subject.

Even although, subsequent to the birth of the child, a fluid, lacteous in appearance, and absolutely so in quality and constituent parts, be thrown off by the uterus, it would not be philosophick to conclude that such fluid was secreted or made by the uterus.

We will illustrate our views by the occurrences and examples of every day. Suppose that a quantity of hydropick fluid is absorbed by the natural action of the absorbents, or by an action of the absorbents excited by means of art; that this fluid is deposited in the circulating channels; and, finally, that it is evacuated

by the kidnies. It would not, assuredly, be contended that this fluid, which has been absorbed from the numerous cells of the body, or the cavity of the belly, was secreted, or originally formed by the kidneys—the organs through which it is evacuated!

The kidnies, in such an instance, could not be said to exchange function with the arteries of exhalation of the cellular substance, or cavity of the abdomen. They can only be viewed in this, as in their natural function, as two great emunctories. The function performed by the kidneys is proper to them; it is their office to throw off that which is recrementitious, or what, if detained, would be hurtful to the economy of the body.

The kidnies, however, are not the only emunctories, by which, on occasion, fluids formed in various cells, cavities, or abscesses, are eliminated. The stomach, by puking, and the bowels, by purging, or the skin, by perspiration, becomes the organ of excretion.—And we have witnessed instances where the uterus, a few days after the birth of children, threw off several gallons of hydropick fluid previously collected in the cavity of the belly, and, by the absorbents deposited into the channels of circulation.

That sometimes one organ, and sometimes another, is pressed into this service for the safety of the general body, is merely incidental. It is possible, indeed defensible, that a fluid, not natural and suitable to the sensibilities of the body in its healthy state, may, by the offensive properties with which it is charged, excite to action any organ, the excitability of which is augmented by disease. The stomach or the bowels, having the excitability preternaturally increased, by indisposition,

or by the impression of medicine, would, on such occasion, became the organ of excretion, of the hydropick or any other fluid not natural to the body. Yet, in this, they perform an office, in its general nature, proper to themselves.

Something analogous to this takes place when the kidnies are acted on by any medicinal articles; every medicine is a poison, on a scale more or less extended. For if it did not disturb the economy of the part or the whole, it would be an aliment, or an inert article, and not a medicine. This approximation to disease, in the disturbed organ, raises its excitability; in other words, diminishes its power of resistance, and hence it is made the channel through which the evacuation is made.—This is the condition of the uterus immediately subsequent to delivery.

Were all the emunctories to retain their condition of health, it would be impossible that any accumulation of fluid could be thrown off, more by one organ than by another. And it is altogether owing to this difference of excitability, between the excretory organs, brought on by absolute disease, or superinduced by factitious measures, that we can have any expectation of a recovery from dropsy.

The skin, the breast, the kidnies, the testes, the liver, the lacrymal gland, all have a peculiar apparatus or structure, constituted by cells and tubes, by which the fluid, secreted by each organ, is carried off. And we are justified in presuming that the uterus and every other secreting body is provided in like manner. Thus, although a fluid, strictly lacteal, were evacuated, we find ourselves at no difficulty in resolving the phe-

nomenon. The milk is formed, and can be formed, only, by that organ the laws of which are suited to its formation. That the fluid secreted by the uterus, during pregnancy, if any be secreted, is really lacteous, is a notion incongruous and absurd. In the first place; the child, at the epoch of its conception, requires a fluid more elaborated; the gross article of milk, would be, by no means, suited to its tender condition. And, again, as remarked above, the fluid, secreted, must be in nature according to the action of the vessels that made it; and that action will be forever, as may be the law of life with which the part is endued.

It would not be more irrational to suppose that the eye would smell, or the nose could taste, than to admit the uterus could secrete milk, or the breast form the catamenial fluid. The faculty of vision is not more distinct from the olfactory function, or this from that of gustation, than is that which makes the milk, from that which furnishes the tender ombryon with aliment.

When the uterus is diseased, and does not throw off the menstruous fluid, it is, not unfrequently, found that a quantity of blood, or something like blood is evacuated from some other part of the body. In the general, if not uniformly, the evacuated fluid is not only blood in appearance, but possessed of all the properties of blood. It is true that the menstruous secretion has, by an annotator, (Dr. Caldwell) on Cullen's first lines, been spoken of as a "*sanguineous secretion.*" Unfortunately, however, for ourselves, the term "*sanguineous secretion,*" lies beyond the limits of our comprehension, we therefore can have no pretension to give it a rational interpretation. We have hitherto understood that the

sanguineous mass—the mass of blood—constituted the source from which all secretions were made. But of what materials, "a *sanguineous secretion!*"—*a secretion of blood,* (this is only translating the term into more simple English, without at all changing its import,) is made or whence derived, we are totally at a loss to conceive. Perhaps, at some future day, the profession may be favoured by the original author of the philosophick conception, with a physiological interpretation of the happy conceit, a "*sanguineous secretion!*"—a secretion of blood!! Really the overwhelming force, and captivating felicity of the conception of a sanguineous secretion, has taken us entirely from our subject. We were engaged, however, on what is generally termed the vicarious menses.

But as the fluid poured out, sometimes, by the stomach, the bowels, or the nose, &c. &c. is coagulable, and possesses all the usual properties of blood, there is no propriety in denominating it a vicarious menses. The real menses, we are informed, by good authority, are not coagulable, nor endued with any of the properties of blood, except a slight approximation in colour.

When we refer to authority, it is to be distinctly understood that we do not allude to the Inaugural Thesis of Dr. Craven, who, the world is informed, by the expositor of Burn's Midwifery, was graduated on this subject in 1778. We can assure you, and also the reader, that no such person was graduated in that year, or any other year, from 1759 to 1785. Nor any other writer, whose work has come into our hands on that subject. On this point, the Thesaurus Medicus Novus, may be consulted. It contains the names of all the graduates

from 1755 to 1785, and also the themes on which they were graduated. We are thoroughly satisfied that no source was open, and accessible to the editor of Burns' Midwifery, until the year 1798, the period at which the thesis, to be found in the physical sketches, was presented to Dr. Rush.

The stomach, accustomed to take in according to the demands of the general economy, still continues to obey its laws and receive the usual quantity called for by the body, not only such quantity as is appropriated immediately to its sustentation, but also for the customary disbursements and losses.

By this means, there is more blood made and accumulated than comports with the safety and convenience of the habit. Some part or organ, in its vessels, is weaker than another; the whole is presssed, the weaker gives way, and of consequence a hemorrhage ensues. This discharge of blood is reduced, as it were, to periodical rule.

Although from disease, the uterus does not throw off the menstruous fluid, yet the ovaries remain under the laws proper to themselves, and, at every full period, both the uterus and the general body, will be influenced by the condition of these organs; especially will any weak or excitable set of vessels. Not only might such vessels yield altogether under such circumstances, and the bloood be poured out, but, if they did not, a condition approaching to inflammation, might occur, as we frequently find to be the fact, and the body is ultimately relieved by the substraction of a given quantity of blood.

Thus, we have given a solution, not unscientifick, we trust, of this action, apparently menstruous, but really

hemorrhagick. Further, were it found to be menstrual, in the fact, we should have no evidence that the action by which it was, at first formed, is vicarious, any more than that the discharge of milk by the bowels, or bile by the bladder, or urine by the skin, should be considered as vicarious, and that it was really made by the organ through which it escaped. The discharge might be viewed, indeed, as vicarious, but assuredly not the function, the act by which the fluid is made. In the several instances, the secretion would be made by its proper organ; and, if our position be correct, could be made by no other.

It is as probable that an exchange of function would occur between the testes and lacrymal gland, and the latter secrete semen, while in return the former would furnish tears, as that the uterus could form milk, either during pregnancy, or after delivery.

Coeval with the science of physick, in all probability, is the doctrine, that clysters thrown into the rectum, especially if they ascend well up the bowel, and alimentary substances applied to the surface of the body, may become nutritious by absorption, but the view we have taken militates very strongly with this doctrine. There is no conceivable power in the lower bowels, or the skin, by which the act of digestion can be performed. If the aliment be not subjected to the primary process of digestion, it is difficult to conceive how it can be finally assimilated so as to become strictly alimonious.

We are, probably, deceived in our views of the effects of nutrimental articles, used as above suggested. The stomach alone can digest; to this organ alone are the laws of association related, which are established

throughout the animal economy, and by which the appetencies for food, are maintained and regulated. Our appetite for aliment and drink are not conditions of the stomach, solely, but of the stomach, influenced, more or less, by relations extended to every part of the living whole. As the disposition in the infant to suck, by the complex structure of the mouth and throat, depend on the conditions of the stomach, so the laws of the stomach, by which the gastrick juice is secreted, have their affinities and connexions with every part of the enlivened organization. No organ acts alone for itself, yet every organ performs its proper office. And it would be as rational and as agreeable, to the facts of physiology, to admit that the stomach or duodenum, could secrete the semen, a fluid for the procreation of the species, as that the rectum, or colon, or skin, could prepare the gastrick juice, the chyme, or the chyle. Except our food pass through the primary process of digestion, it is scarcely possible that it could ever be appropriated to the purposes of the animal economy; each subsequent part of the general process, depends, in a great degree, upon the precedent; without digestion, there can be no alimentation.

Nutritious articles, applied to the skin, or thrown up by the bowels, may, however, be indirectly advantageous, not as alimentary substances, but as means by which a certain degree of stimulation is kept up throughout the system, and thereby supporting an increased action of the whole. By such auxiliary measures, the assimilating powers would be enabled to finish, more completely, their operation, and thus assimilate, more perfectly, such nutrimental substances, as had passed

through the stomach and been subjected to its process, together with whatever changes may be superadded in the duodenum, &c.

In this way, we view the practical advantages resulting from the use of what are called nourishing clysters, and nourishing baths. That the body, when much exhausted, is revived; or that life is actually prolonged under the use of those measures, a practical man may not doubt, and a correct science, will, we are persuaded, give every practical physician, views similar to what we have offered. Could membrane, or muscle, or bone be made without blood? Surely not. Why then presume that blood could be made without chyle. And is chyle made by other organs than the stomach and duodenum?

TRACHEOTOMY SUPERSEDED BY NATURE.

DOCTOR DAVIDGE,

I now send you a more minute statement of the case, which I incidentally mentioned to you in the course of conversation, when I had the pleasure of seeing you in Baltimore, and which you considered of sufficient importance to narrate to the class, in your interesting lecture on Tracheotomy, which you politely invited me to hear. The case, so far as my reading extends, is without a parallel; if you think it worthy of being recorded in your proposed Journal, (which I trust may succeed according to your warmest expectations,) you are at liberty to publish it. A negro child 14 or 15 months old, in the neighbourhood of Hagers-town, whilst sitting playing on a bed filled with *cut straw*, was seized, without any apparent cause, with a violent fit of coughing; indeed so severe was it, as to induce its mother, who was present and who immediately took it up, to suppose that it was "entirely gone," as she expressed it. This strangling, however, soon passed off, and nothing remained but a slight occasional cough during the three or four following days, when the cough became increased, the breathing obstructed, the child screaming, as from violent pain, whenever it was moved; fever, and in short all the symptoms of Pneumonia, presented themselves. These were relieved by venesection, blisters

to the thorax, and a strict observance of the antiphlogistic regimen. The child now appeared as well as usual, except an occasional cough, and no more attention was paid to it for seven or eight weeks, when the mother observed a swelling on the side, situated about three inches from the *spine*, and over the *sixth* and *seventh ribs*; in a few days a fluctuation of pus was perceived in the tumor, and an opening into it was made with a lancet, when together with a large quantity of pus, part of a *head* of *Rye* about two inches in length, was discharged. Now all the circumstances of the case were unfolded; the sudden attack of coughing and suffocation, the symptoms of Pleurisy, and finally the abscess were accounted for by the presence of this foreign body, which was no doubt drawn out of the bed by the child, and in attempting to swallow it, it passed through the *rima glottidis*, into the trachea, from thence into the air cells of the lungs, and then made its way to the surface of the body. That it passed into the *trachea* and not into the *œsophagus*, is manifest from the succeeding symptoms, and from the situation of the abscess, *above* the diaphram. If it had been swallowed, there would have been no danger of suffocation, no symptoms of inflammation of the lungs and pleura, and the abscess would have been situated *below* the diaphram, in fact, in my opinion, there would have been no suppuration at all, as the powers of the stomach would certainly have been sufficient to digest the substance, and it would have passed off through the intestinal canal. It is certainly no very difficult matter to explain the process by which this substance was expelled. After the irritation, caused by its passage through the glottis and trachea, had subsi-

ded, a slight cough was kept up, until inflammation was excited, which eventuated in suppuration, and the foreign body, in obedience to a general law of nature, (call it vis medicatrix naturæ, or any thing else you choose,) proceeded towards the surface. The symptoms did not continue so urgent, in consequence of the depletion and reduction of the inflammatory diathesis, but a constant irritation was sustained as evidenced by the cough which remained. When it reached the surface of the lung, adhesion, produced by the inflammation, was effected between the pleura pulmonalis and pleura costalis, and thus an easy exit was given to it from the body. I would also observe, that independent of the tendency which foreign substances possess, of working towards the external surface of the body, from the particular structure of a head of rye, if it entered the lungs *but end foremost*, the perpetual motion of the lungs would have a mechanical effect in propelling it forward. That it did pass through the lungs *but end* foremost, I infer from the fact, that it was expelled from the abscess in that manner.

I am, respectfully,

Your friend,

SAMUEL SHUMAN, M. D.

Hagers-town, (Md.) 27th December, 1822.

A MEMOIR ON

FRACTURES OF THE THIGH-BONE.

BY J. B. DAVIDGE, A. M., M. D.

USU COMPERTUM HABEO.

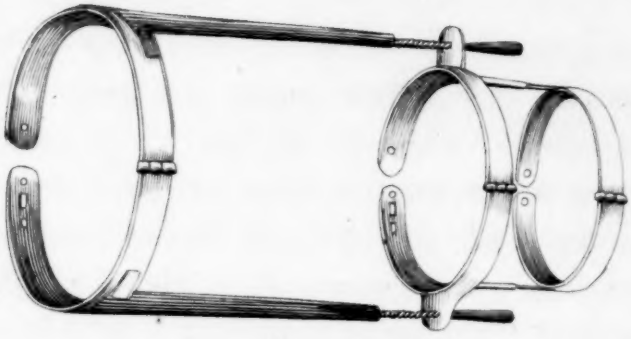
Few, or no subjects, have more frequently, or more imperatively called on the attention and ingenuity of professional men, than fractures. By the frequency of their occurrence, and their importance, arising out of the difficulty with which they are managed, especially those of the oblique character, when of the clavicle or thigh bone, or although transverse, when of the neck of the thigh; which is always oblique in relation to the axis of the limb, in consequence of the manner in which the neck is deflected from the course of the shaft, they demand the most serious consideration. And nothing more strongly argues the difficulty of management, than the great variety of machines, and multiplicity of apparatus, brought forward in the different ages of the world, by the various and sedulous attempts at success. To enumerate the variety of machinery, or detail all the complexedness and contrariety of bandage and apparatus, would be to lay under serious contribution, the patience and memory of the reader, without instructing his judgment, or illustrating the subject. It may suffice to indicate the more prominent; refer to the individual surgeons by whom they were invented or

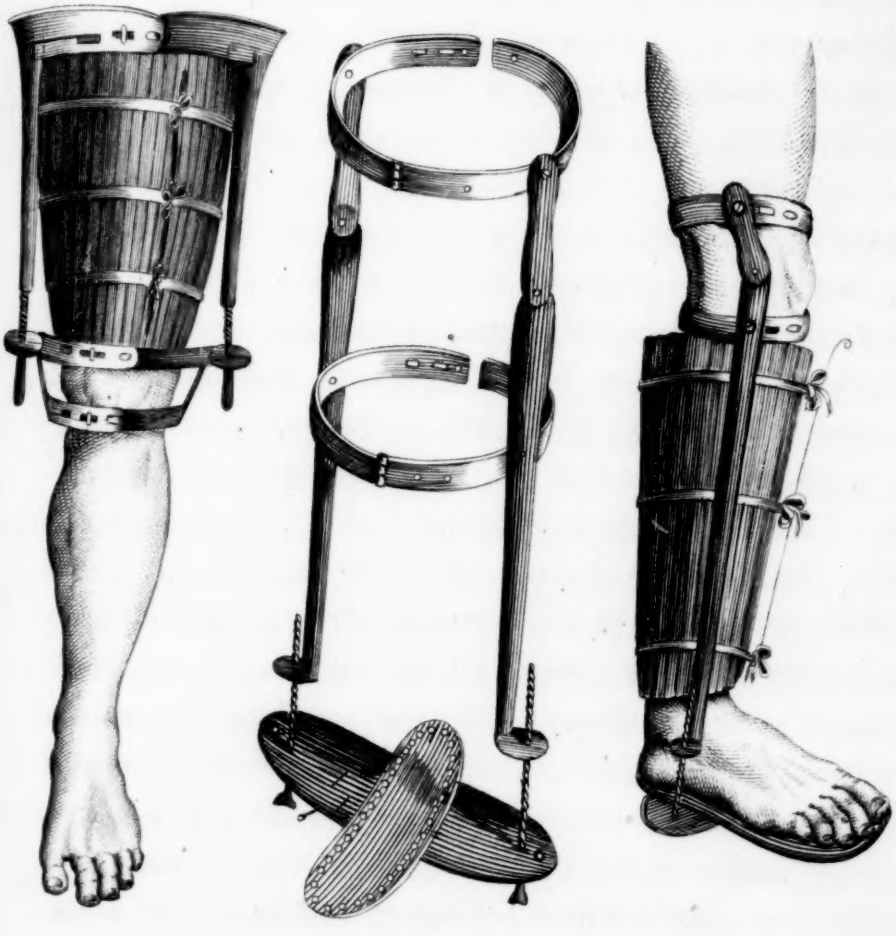
continued in use; and, by way of facilities to the comprehension, add a few plates or diagrams. Description, aided by a sensible representation to the eye, will, we hope, supply every thing necessary to the elucidation of principle.

We first present to the examination of the reader, the splints under the representation of the plates, which, having examined, he is better prepared to understand their manner of application and principle of operation; and, as nothing tends more strongly to impress on the recollection, things and discoveries, than some particular, but not laborious, sketch of their history, we shall follow the plates with a limited historical account from the writings of Heister and Bell, the two most able and best informed writers on the subject.

Heister, writing on the bandages for the fractured thigh and leg, after having detailed the various bandages first to be applied about the limb, goes on to remark that, "the deligation being in that manner completed, the next business is for the surgeon to place the thigh in the most convenient posture, for which we use a kind of mattrass, or straw bed, furnished with two cylindrical sticks, (flat boards will answer as well,) with straw, as in table IV. (of his work,) and fig. V.; only here the two sticks, or junks, must not be of the same length, as they are for the fracture of the leg, or tibia, for which this figure is adapted; for that going within side the leg and thigh, should be just long enough to reach from the internal ancle to the inguen, and the external one, to reach from the thigh, *or superior part of the os ilium*, to the external ancle; or, *as some will have it, long enough to reach from the said ancle, all along the side*

Gooch





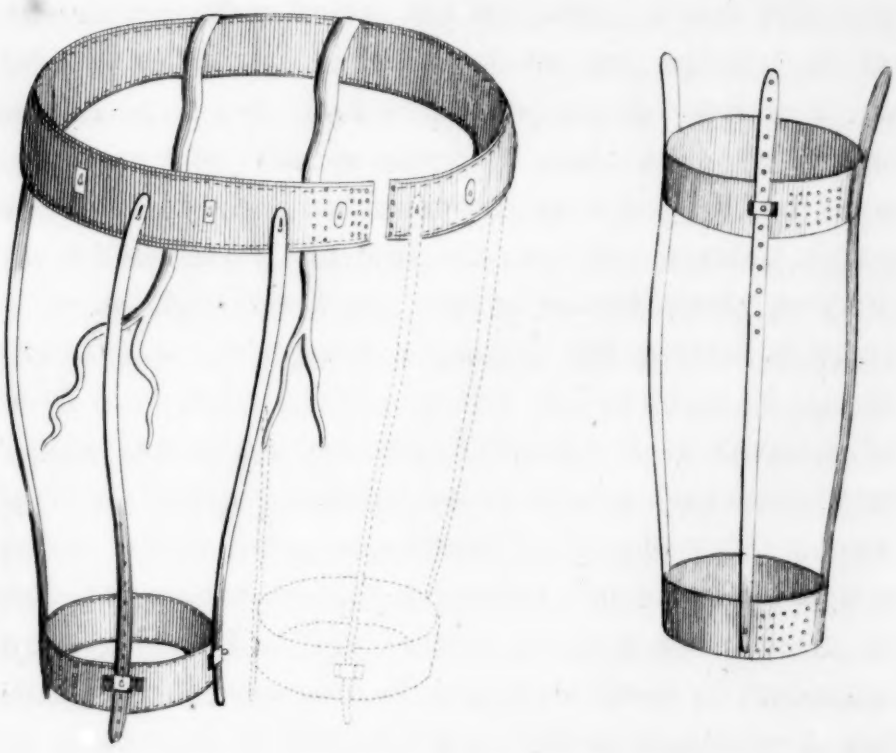
f the body, to the axilla ; for if these supporters are not long enough, in an oblique fracture of the thigh, there is great danger of its contracting and becoming shorter than the other, which will necessarily subject the patient to halt in his gait. However, M. Petit will not have the external one reach any higher than the upper part of the hip, (or the superior line of the os ilium,) which will prove always sufficient, provided the rest of the deligation be tight." Heister, vol. ii. p. 328.

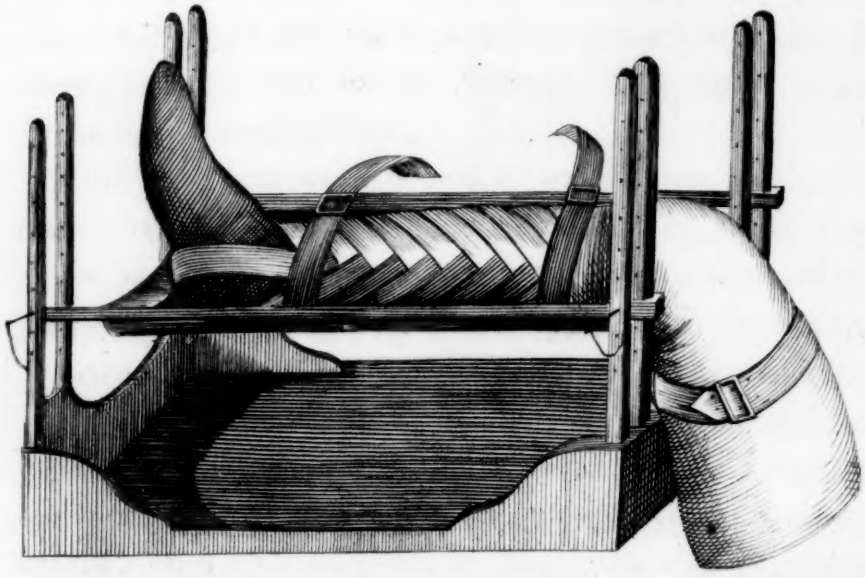
" Nothing can be more surprising, than the similarity of those machines"—Bell is writing on the machines of Gooch—Aitkin—and Wathen,—" unless it be the anxiety of the several authors to be considered as the original inventors. These authors exclaim in bitter terms against their contemporary writers, each accusing the other of plagiarism, of stealing his invention ; but their mutual accusations are, after all, but a verification of a good old proverb, which we need not repeat.—Aitkin has, in fact, stolen from Gooch, Wathen from Aitkin, and all of them have stolen from Hildanus ; no wonder, then, they found out each other's thefts." Bell, then, having compared the machines of Gooch, Aitkin, and Walthen, with the invention of Hildanus, proceeds, " unless I had reasons to think the principles wrong, I should not thus critically enquire into the history of those machines. The principles of their invention, are two, only ; first, to institute some steady resistance to the contraction of the muscles ; or, in other terms, to keep the limb in perpetual strain ; and, secondly, to make the pressure at distant and fixed points, or in other terms, to apply the whole of the force necessary

for steadying the limb, upon particular parts of it, while the rest of the limb is uncompressed, and unsupported. But let us quote, impartially, those aphorisms in which the principles are laid down. First, it is affirmed that the cause of the retraction of the fractured bones, is the contraction of the muscles of the thigh ; that this contraction must be opposed by some *steady* and *permanent* resistance ; "that the grand desideratum is to create a temporary substitution to the now deficient opposition which was afforded by the thigh-bone to the contractility, (contractibility,) of the femoral muscles ;" words which sound well, and have made no small impression. Now, the muscular contraction is a power which never ceases to act ; which is, indeed, continually increasing, while the hold of the circulars upon the knee and ancle is continually lessening. The muscular action is rather excited by being opposed, and when excited, is irresistable. It is this power which breaks the bone in fracture of the patella, and tears the tendon in rupture of the tendo-achillis. How, then, should such machines resist it? But even allowing that such machines have power to resist the muscles, and strength enough to preserve the extension, how shall we enable the *soft parts* to *endure* such force, or how give them a perfect hold upon the limb? make but soft parts uncompressible, so as to give a permanent hold, make them as hardy as those iron bands, so as to bear to be grasped, and I will acknowledge the most powerful machines to be the most perfect."

Secondly, it is laid down as the essential principle in those improvements of Gooch and Aitkin, "that this substitution to the resistance of the bones, can be

AITKIN'S improved on **GOOCH**





Fracture-box *for the Leg*

obtained only by assuming two fixed points ; the one above, the other below the fracture. From this very principle, so well established in the opinion of the inventors, result, in my opinion, all the defects of the machines ; for that pressure is made at three distinct and distant points, which might have been diffused over the whole limb ! In order to exert but a small degree of force, the ancle, knee, and pelvis are sorely pressed ; there is at those points a partial and painful stricture, with intolerable galling, which the padding of the circulars can never prevent ; the limb rests upon no flat surface to keep it steady, but is wholly insulated, those points alone being compressed : in place of a great resisting power, which might have been easily obtained by a general bandage, which gives to every point, its share of pressure, and of course its share of resistance, no more than a very slight resistance is obtained, and that too, by squeezing the knee and ancle in these iron circulars, with great and intolerable galling. The limb is thus held continually, as in the grasp of two cruel hands, for six weeks, and while the heads of the bones are squeezed with intense firmness, the middle fractured part is left unsupported."

But of all arguments that of experience is the fairest, and the machines have now been tried by time, that severe justicer, who respects neither authorities nor names. There is no reason why I should mention to you the experience which I an individual had, since the *total disuse* of these machines, expresses very unequivocally, the opinion of the whole profession ; but I venture to represent to you, the contentions I once entered into, with a stubborn and sturdy highlander in our hospital,

who had fractured the neck of the thigh bone, I think it would cure you of all desire to repeat the trial. I have on several occasions, tried these machines, with that impartial zeal with which it becomes us to receive any suggestion, especially in a case of such acknowledged difficulty. I have reinforced Mr. Gooche's and Aithins' machines with additional buckles, belts, and screws, but I have screwed my patient into a state of indiscribable torture, in which neither he had the courage, nor I the cruelty to persevere."

Perhaps there cannot be a more awkward situation, than that of the surgeon, who, trusting to the unqualified encomiums of the authors of those inventions, has congregated his carpenters, smiths, and saddlers, and bellows-makers, and set them to work! and after screwing his patient into a state of exquisite torture is forced, with shame and confusion, to give up the point, and lay out the limb smoothly upon a pillow to recover from the galling and bruising of his machines. It is then only, that he begins to perceive the wide difference between speculation and practice, betwixt a machine put about a sound limb, and a bruised and fractured one; it is then he becomes sensible how easily an untruth may be told in a drawing."

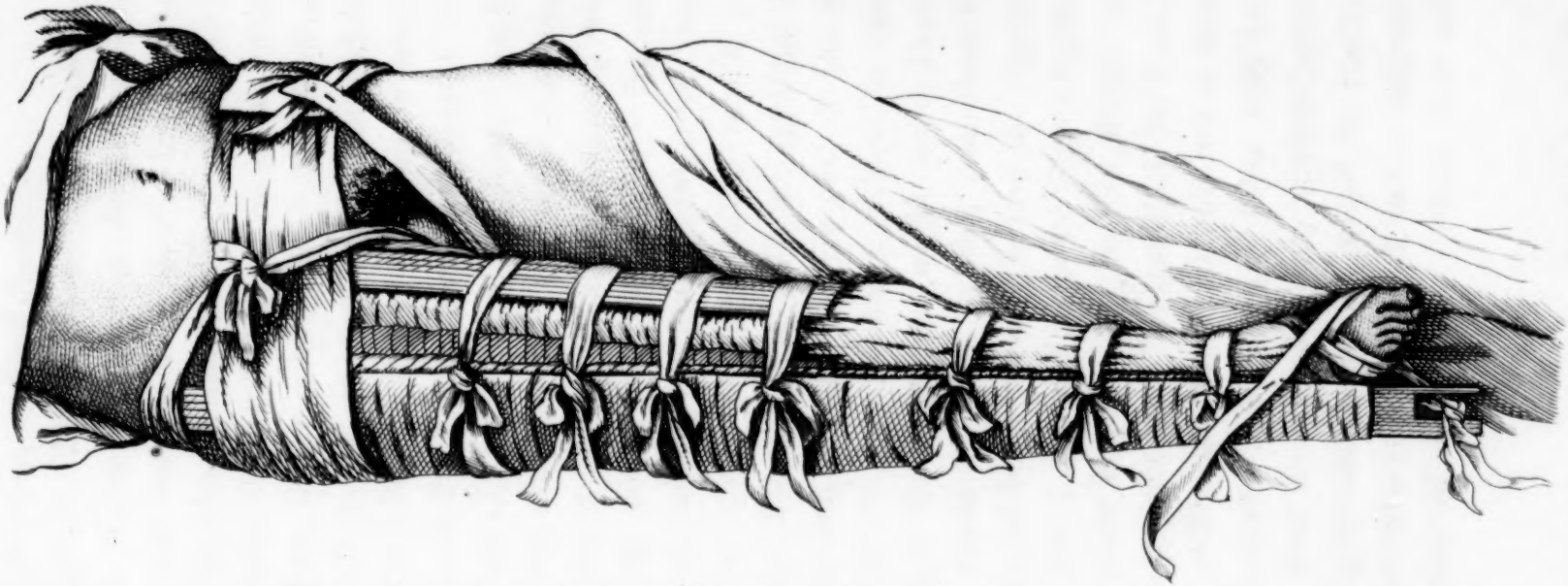
"Next in order is the method used by Mr. Dessault for extending the fractured thigh. I wish sincerely, I could speak of his invention with a degree of respect, at all proportioned to the deference I feel for one who was undoubtedly a great surgeon, diligent in his practice among the poor in the Hotel Dieu, and kind and encouraging to the young surgeons, who served under him, and who in return testified their gratitude by

writing out his cases and observations. His theory is unworthy of his high character; his intentions, and indeed his *very words* are anticipated, not merely by old surgeons, whose works he might have neglected to read, but by his immediate predecessors and contemporaries, Petit, Sabatier, and Duverney. He was in the custom of applying the long splint of Duverney, with as much anxious preparation, as if he had trusted to that alone, while his peculiar method, that on which he entirely depended, consists in fixing the patient to the head, and drawing down the heel towards the foot of the bed by lace or bandages fixed round the ancle, a method neither original nor successful, not even consistent with the splint, which he applied at the same time."

"First, Mr. Dessault applies round the broken thigh-bone, (supposing it broken in the middle,) one compress above the place of fracture, and one below it; over these compresses he applies an eighteen-tailled bandage, (afterwards changed to the roller or Scultets,) and then taking two large pads or cushions, he lays them along each side of the thigh; thus far he complies with the rules of the veteran surgery—he binds up the fractured thigh with these compresses and cushions, to fill the hollows of the splints. Next, he applies a long splint, about three inches broad, and reaching from the heel to the haunch, or beyond it, while he lays a shorter and a smaller splint, along the opposite or inner side of the thigh. The long splint is secured by being let in betwixt the folds of a table napkin, bound firmly round the pelvis, while both splints are made firm by bands of ribband, or tape, tied round the whole."

"Here is nothing different from the method of Heister and others, but it seems a mere compliance with the customary practice; he troubles, or rather tortures, his patient, with a complicated apparatus, in mere compliment to the old surgeons, and then, with all possible sang-froid, concludes, 'that yet nothing can effectually oppose the displacement of the thigh-bone, except such an apparatus as shall prevent the trunk gravitating towards the thigh, and the leg, on the other hand, being retracted towards the trunk.'

"This is the peculiar theory of Dessault, and the additional apparatus which he next applies, he boasts of as his own invention, and as the constant and successful practice of the Hotel Dieu for three years. The roller, the cushions, and the long splint of Duverney being applied, Dessault is next to prevent the gravitation of the body, (or in plain terms, the trunk sinking down in the bed,) and the retraction of the leg. First, he puts a large folded cloth, napkin, girdle or roller, round the thorax, just under the arm pits, to which is fixed those bands by which the patient is tied up to the head of the bed, and the body prevented from sliding down, from descending or gravitating towards the fractured limb. 'Next,' says Mr. Dessault, 'nothing is more easy than to prevent the thigh being retracted toward the pelvis,' and for this purpose, the ancle being encircled and defended with a long flat compress, laid round the back of the leg, and round the ancle, the middle of a common roller or bandage is to be applied round the heel, turned round above the ancle, crossed upon the fore part of the foot where the buckle lies, and



The shorter Splint of the French and Germans

tied over the sole of the foot; this roller carried to the foot of the bed, is fixed there."

Thus does Mr. Dessault accomplish that perpetual tension of the limb, which he decorates with the fine name, "permanent extension"—but how he can regard this as peculiar to any period of surgery, as the exclusive practice of the Hotel Dieu, or as an invention of his own, I am at a loss to divine. The doctrine, (since we must call it a doctrine,) was announced by Mr. Petit in a very formal manner, and he was accustomed to fix the lac, or ligature, which is designed for preventing detraction of the thigh, to the foot of the bed, while a large table cloth, fixed to the top of the bed, and passed round the groin, supported the patient, and prevented the trunk descending towards the fractured limb.—There is, indeed, nothing peculiar in the method of Dessault, except the precautions which I shall now mention. First, that in place of applying his bandage for extension, round the lower part of the thigh, where the veins and arteries being subject to compression, a swelling of the whole limb might ensue, Dessault applies his lac round the ancle, where, from there being less of the bulk of the extremity below the ligature, and the vessels being defended in some degree by the projection of the ancle bones, there is less danger of swelling. Yet this precaution of Dessault's is far from being either new or effectual; for Hildanus, when instructing the young surgeon how to apply these very lacs, acknowledges, that though commonly applied round the knee, they may be better applied round the ancle. And Mr. Sabatier, who wrote a paper on the subject, before this publication of Dessault's, proves that not only this

permanent extension has been at all times a very common practice, but also that it is a very dangerous one. "The consequences," says Sabatier, "of this method, are so distressing, that nothing can persuade me that it is not very dangerous; for the thigh and ancle are prodigiously swelled by the compression of the lacs, while the parts are excoriated by the filth of the urine, and the ordure with which the bands are continually soiled; while some have suffered much from continual fever and irritation, others have died in unspeakable torments." These are strong testimonies against this practice; they are plain representations of what Sabatier had seen; but they are not mentioned by Dessault, who, with particular address, and, I am sorry to say it, disingenuity, passes over these testimonies, while he quotes Sabatier on every other point, and leaves us the ungracious task of confronting Dessault's eulogiums on his permanent extension, with these unquestionable facts."

"Secondly, Mr. Dessault, as he had removed the lower bands from the knee to the ancle joint, removes the upper band from the pelvis to the thorax. When Petit and other surgeons practised the same method, the patient was supported merely by a table cloth or sheet, passed round the groin; Dessault alleges that this would gall the patient; but Dessault's own bandage round the thorax, produces not galling, (which a man of fortitude could easily bear—which a man in these circumstances, must bear,) but oppression and insufferable distress, which no one can possibly bear. Of the difficulty of supporting the anxiety produced by this stricture round the thorax, Dessault's own cases give

most unequivocal proof; his patients often were so oppressed with their bandage, and breathed so difficultly, that he was obliged to ungird them; and, in one case, the relief expressed by one of the patients, is recorded by Dessault himself, in very strong terms, though we may venture to suppose that the patient would have used still stronger expressions. Dessault says that when he exchanged the bandages and permanent extension for the long splint, the man was no sooner unharnessed of the girths, with which he was bound to the head and foot of the bed, 'than the pain of the thigh ceased, and he breathed more easily;' but had the man been permitted to dictate his own page in the journal, he would, I dare say, have declared, 'that he was in heaven, the moment he was unbound.' I perceive Mr. Dessault is unwilling to acquaint us, all at once, with the whole truth; for we find, by circumstances mentioned on another occasion, that the patient was so bound down, that they were even unable to slip any thing under him to receive the natural discharges. His posture was as terrible as that contrived by the old lithotomists, who tied their patients not only by the hands and feet, as we do, but literally hilt to joint; for, besides the bandages which fasten the hands to the feet, they had another great bandage which went from the heels round the neck, like a cobbler's strap, and tied the patient double."

We shall pass on to Bell's concluding paragraphs, as the reader, no doubt, has seen enough to satisfy him that Mr. Dessault has no claim to originality in regard to the longer or the shorter splints, which we formerly, and properly termed, the long splints of the continent.

It would be difficult to determine at this late date, who was their inventor.

"I have proved," concludes Mr. Bell, "this interesting lecture, in a former discourse, that a thigh be easily extended from time to time. In this present discourse, it has been observed, that what has been called permanent extension, is actually an extension renewed from day to day. When the thigh is laced in the way now suggested, we have the fracture always under our eye; we do not go through the cruel and formal operation of extending with lacs and pulleys, and numbers of men pulling upon a broken limb which we are sensible we cannot retain in its extended position, we stretch it gently, model it with our hands, lay it out smooth, stretch and replace it, from time to time. We now find, by experience, where force is required, it is useless; that it is only where force is not required, that we succeed; we find, that after a gentle extension, the limbs of boys and girls, or of women, of weakly subjects, and of old people, lie pretty steady; we find that occasional extension, corrects every occasional contraction; we have no difficulty except in the thigh of a strong and muscular man; and we find that after buckling the most powerful machines about the thigh of a strong and muscular man, we are invariably foiled, and obliged to desist. It is only a big and unwieldy thigh that is much retracted, and, we find by experience, that after some time the strenuous contractions of such a thigh, subside; its irritable resistance to our gentle extension, ceases; it falls into a quiescent state, allowing itself to be smoothed and gently drawn out, and laid along on its pillow! I have often observed that

a big and muscular thigh, at last, settles down as it were, in its place, and takes a seat and posture, so that it is not easily discomposed by any accidental spasm, contraction, or unwary motion of the patient."

"These explanations I owe to you, as young and unexperienced surgeons, and will venture to foretel, that, after being like others, seduced to use powerful machines, to this simple process, at last you will return. The fractured thigh-bone, seems, in a strong man, to be retracted by natural powers, which nothing but rude force would successfully oppose; surgeons have delighted in force; their machines are but too powerful! We know by experience, that the resolution of man cannot sustain the tortures of a limb, extended by machinery, for weeks. We question not the power of machinery in extending a fractured limb, but it is much to be questioned whether the system be able to suffer such protracted torture; if it be, it is at least a phenomenon which I have never witnessed, nor expect to see.—Wherever extreme violence is required, I hold it to be a sign that violence will not succeed."

"It is my privilege, while I deliver the history of the machines, to express my opinion, to say how imperfect I think the principles are, how impossible it is to resist the contractions of a powerful and muscular thigh, because we are limited, not in respect of the extending power, but in the properties of the soft parts of the limb to which such power is to be applied. Bring me *one* thigh-bone of a full grown man, fractured in the neck, and not shortened during the cure—bring me *one* bone so united, either before, or since the invention of modern machines, and I consent that you torture your

patients with screws, rods, bolts, and pulleys, till you be weary of cruelty, and convinced with Platner, "*that the thigh-bone fractured in its neck, can hardly be preserved of its natural length.*"

Dr. Dorsey, in the first page of his preface to the Elements, remarks, "that Great Britain and France have been foremost in the cultivation of modern surgery, but their deficiency in philosophick courtesy and candour, has, in some instances, greatly retarded its progress—to illustrate this remark, it will be sufficient to state that the doctrines of adhesion so ably developed in England, have been shamefully neglected in France; and that French surgery, in fractures, finds no advocates in Britain. Some of the best writings of Dessault, have never been translated into the English language, and those of John Hunter are unknown, or disregarded throughout the continent of Europe."

When the author of the Elements of Surgery, in the paragraph immediately precedent to the one from which we have made our quotation, acknowledged his inadequacy, it was not expected, that so early as the paragraph immediately following, we should meet with a verification of his acknowledgement, that, the labour of giving "an American Epitome of Practical Surgery, had devolved on one, in many respects inadequate to the performance."

We flattered ourselves that this acknowledgement of inadequacy, was rather the suggestion of diffidence and decorous modesty, than the serious dictate of professional conviction. But when we read of the retardation of modern surgery, by the deficiency of philosophick courtesy and candour, between the British nation and

the French people, we were disabused of our mistake. We had hitherto admitted, that the field of science, surgical, or any other, is never so successfully cultivated as by the sharp implement of a strong, bold, and keen criticism. Personal and social courtesy have their value, but when the stern rules of philosophy, and the sacred obligations of science, call on us, we may dispense with the courtesies of the drawing room, and France may not feel herself bound to recognise that as English, which was, by herself, taught to Britain. The doctrine of adhesion was borrowed from France—that Britain outstripped France in the race of its application, is possible.

"The French surgeons, who first invented the flap amputation, declared, not only that their flap amputation procured an easy and perfect cure, but often, in three days, the flesh of such a stump had adhered. To this, O'Halleran replied, with a rudeness and ignorance, quite unparalleled." John Bell, v. i. p. 42.

Had Dr. Dorsey, before he found it convenient to become an author, made himself acquainted with surgical history, he assuredly could not have censured the French for not acknowledging themselves indebted to the British, for any thing in regard to the invention of the manner of healing wounds by the first process.—Indeed, he would have known, that both the French and English derived it from Celsus, who certainly alluded to it, where he says, "that amputation, as to its flaps, is to be cured in the same way as other wounds in which pus is not to be promoted." The same kind of historical knowledge would have saved him the trouble of complaining "that the French surgery, as

regarded fractures, found no advocates in Britain."—The reason is very obvious; the British surgeons had enough to do in getting rid of their own follies in regard to splints, without taking into account the far more useless and mischievous of the French. That "some of the best writings of Dessault have never been translated into the English language, and those of John Hunter, are unknown, or disregarded, throughout the continent of Europe," admits of many apologies.—First: Dessault, in all probability, never wrote any thing—it is generally understood that his opinions and practice, were given, in the form of a journal, to the world, by his pupils in general, and that production, which is called his works, by Bichat in particular. But there remains still a stronger and a better reason; Dessault possessed no opinions proper to himself, in regard to splints for the thigh; we are writing on the fractures of the thigh; they were all taken from Duverney, Petit, and his contempories, together with Heister; and the first use Dessault made of Duverney's splint was to render it more exceptionable, if possible, than it was in the hands of Duverney himself. In his latter days, according to Bichat, Dessault gave up Duverney's splint, which reached to the arm pit, and was secured by a strong band round the breast, and another round the pelvis, and adopted that of Petit, which reached no higher than the spine of the os ilium, adding, perhaps, to the length of the splint, and making it extend a little beyond the foot

Would it have been decent for the British to employ themselves in translating the paltry piracies of Dessault, or his pupils, when the originals were all in

their own hands! Moreover, translations are not altogether necessary for the British professional men—they, in the general, read French with nearly the same facility they do their mother tongue. Therefore, that the best writings of Dessault were not translated into English, is no evidence they were not read, and understood, by the British.

But to come more immediately to the business before us. The author of the Elements says, "the principal difference between the mode of dressing a fractured thigh, just described, and that recommended by Dessault, consists in the increased length of the splint by which the permanent extension is kept up; an improvement which was first made by Dr. Physick." "In the first place, the splint of Dessault being but little longer than the limb, the strap, or band, for counter-extension, passes over the upper part of the os femoris, at an oblique angle, and has a strong tendency to draw the superior fragment of the bone, outwards; whereas, in the splint contrived by Dr. Physick, the mortise hole being cut high up near the axilla, in a line nearly parallel with the natural direction of the limb, and has no tendency to any lateral displacement of either fragment. In the next place, by extending the splint as high as the axilla, *part of the counter-extension* can be sustained at that place, although the chief dependence is to be placed on the pelvis." Elemt. v. i. p. 162.

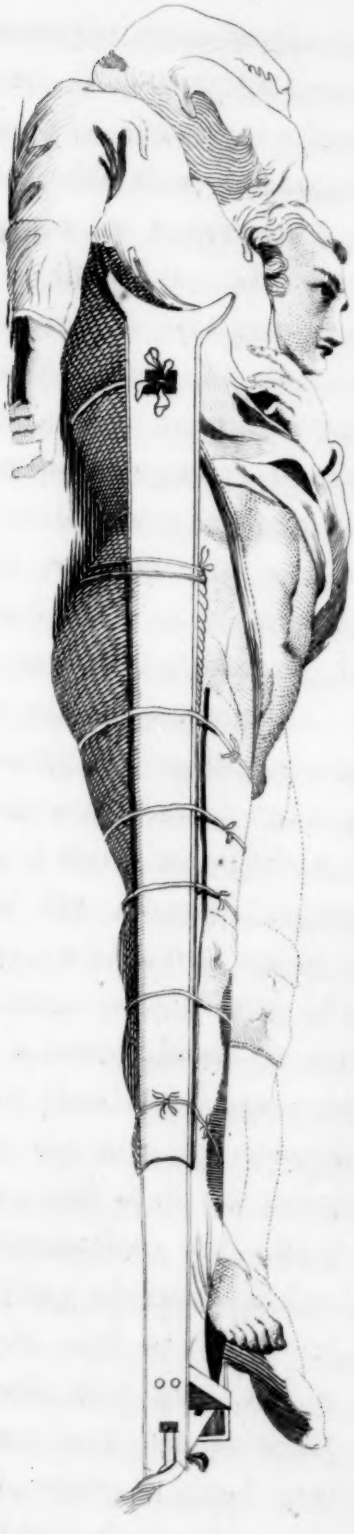
The reader, by turning back to the quotation from Heister, will perceive that in his day, long before the period in which Dessault copied from Duverney, the longer splint, or in other terms, the outside splint, occasionally, was so long as to extend to the arm pit.

It is true, not that a part of the counter-extension should be sustained by the shoulder, a moveable point, and by consequence, could not possibly be a point of support, or bear any part in the counter-extension. That a moveable point could sustain a part of the force in the counter-extension, was left to illustrate the mechanical genius of Dr. Dorsey and his admirers.

We are again thrown back upon our jealousies of the historical knowledge of the author of the Elements. Assuredly, that important passage of Heister, a writer who should be in the hands of, at least, every man who undertakes to teach surgery, either orally, or in a written manner, ought to have been within the reach of the author's reminiscence. "And the external one to reach from the hip, or superior part of the os ilium, to the external ancle; or, *as some will have it, long enough to reach from the said ancle, all along the side of the body, to the axilla.*" Heister, v. ii. p. 328.

With what sort of feelings, or information, could Dr. Dorsey assert that the increased length was first made by Dr. Physick; in other words, that the longer splint of the Germans and the French, never reached up to the axilla, before this respectable surgeon of Philadelphia extended it so high, in order to save the upper fragment from the consequence of an oblique pressure, and for the advantage of obtaining from the axilla, some aid as a fulcrum, thus dividing the force and effects of the counter-extension?

The anatomical surgeon would be at considerable loss, to understand in what manner any obliquity of the belt, acting on the tuberosity of the ischium, could displace, or force outward, the upper fragment. From



The longer Splint of Germany & France, with the crutch-head superadded by D.r Physick

the tuberosity arise the semi-tendinosus, and semi-membranosus muscles. The gracilis arises higher up toward the pubick bone, and the great adductor along the line between the latter and former muscles. All those muscles are attached on the lower point of the thigh, or on the upper point of the tibia; when the limb is extended, those are drawn into a straight line, of consequence, the belt is thrown rather on the inner angle of the tuberosity. How, under such relations and circumstances, could the upper fragment be forced outward?—Except the belt could press in upon the upper fragment, it certainly could not alter its relation to the lower; that it does not act on it, is certain, and cannot, except the muscles, enumerated above, be lacerated, is clear to every surgeon at all conversant with anatomy. It appears then, that by having the mortise higher up, the splint redresses an evil that is anatomically impossible in its occurrence; a thing merely imaginary.

In the return of Dr. Physick, to the use of the long splint of Duverney, which reaches to the axilla, we can discover neither improvement nor advantage; the terminating it with a crutch-head, is only adding to its inconvenience; the shoulder is moveable, and cannot be a fulcrum, except the arm be secured along down the side of the splint, and then the pressure in the axilla would arrest the circulation, and palsy the nerves going down from the axillary plexus. Indeed, from the plate, as frightful as it is, and so completely in character of a man on the rack, that one, not a surgeon, might mistake it for some favourite measure of Damien, we may infer that Duverney's good sense and extensive experience, saved him from the serious and terrible

consequences of making it a fulcrum, or giving to it any part in sustaining the counter-extension.

With the recurrence to the use of the buck-skin, or shammy band, we should be pleased, had it been accompanied by an acknowledgement, that it was a favourite band some fifty years ago, with Gooch and Aitkin, and is particularly alluded to by Benjamin Bell, in his explanation of the plates, respecting the fractured thigh. The buckskin band, undoubtedly, is to be preferred to any other band, or lac, made of whatever materials it may be. Its surface should be extensive, of consequence, its friction and pressure would be more diffused, and, in proportion as the pressure is extended, as to the surface acted on, will be the diminution of the galling and stricture. The transum and sole, taken also from Gooch, Aitkin, and Benjamin Bell, are complex and altogether useless—a pair of lateral lacs, one going to each of the foot posts, or to two holes in the foot board, will answer the purpose much better, as the reader will perceive when he gets on to where the fracture bedstead is described.

As the Baron Boyer succeeded to Dessault, and has also published some remarks on the method of fractured thigh, it may not be uninteresting to the reader that we give some account of the opinions of this respectable surgeon.

The Baron having promised a short account of Dessault's manner, and that of a few of his cotempories, goes on to detail the peculiarities of his own splint and the method of its application. In principle, the splint of Boyer does not differ essentially from that used by Dessault in his latter days. They both are secured at

their upper ends by a belt that passes over the groin, and round on the tuberosity, so as to act on the tuberosity as their fulcrum—at the lower end they are fastened to the ankle by a band that descends obliquely, and is secured to the lower end of the splint, which projects a few inches below the foot. The most remarkable difference is, that, by Boyer's, the extending force is exerted by a screw which works through a nut fixed on the splint, while that of Dessault's is fastened, and, occasionally, tightened by a simple knotting of the bands.

Having ended the detail, as regards the peculiarities of his splint; spoken largely, and more confidently, than prudently, of permanent extension, he winds up his chapter in the following singular paragraphs.—"The counter-extension cannot be made parallel to the axis of the limb, without putting a splint on the inside, the pressure of which, upon the ischium, experience has proved to be insupportable—neither are there any means of rendering the pelvis perfectly immoveable, for the bandage which is passed around it and the upper extremity of a long splint, does not effect this object. Lastly, the calls of nature prevent us from keeping the limb *perfectly at rest.*

"Hence, the fragments become more or less displaced, even after the application of the apparatus—their re-union may be thus retarded, and it is never perfect in the ordinary time—and, even after several months of rest and judicious treatment, when the limbs appear equal, and the re-union of the fragments solid, the moment the patient begins to walk, the *limb gradually becomes shortened.* There are, moreover, persons, whose skin is so delicate and sensible, or possesses so

little vitality, that compression causes *insupportable pain*, or *mortification of the integuments.* From the first of these causes, *permanent extension* may be *inapplicable* to women; and from the second, to *aged* persons, and those who are *exhausted* by previous disease."

And this is the sum total of all the fine parade and speculations about *permanent extension ;* that in some, the bands and lacs produce insupportable pain, and in others, *mortification* of the integuments ; it is inapplicable to women, and destructive to aged persons ; the delicate and exhausted cannot bear it. On whom then is it to be applied ? On the healthy, or the dead ?

Again, the Baron writes, " Although some persons cannot support the degree of extension necessary to give the limb its natural length, yet such a degree of force as will prevent *great displacement* of the fragments, can almost always be employed with advantage, even though, in some few instances, *rotation of the limb outwards be all that we can remedy.* Lastly, if *continued extension be insupportable,* or inadmissible, even in this moderate degree, we may employ the ordinary treatment for fracture of the body of the femur." That is, as John Bell says, lay the limb out on a suitable bed, smooth and easy—extend it day after day ; prevent the rotation outward, and the patient will do well ; be saved from *insupportable pain and mortification* of the integuments, and, sometimes, we may add, from death.

Not only so, but, " The bandages also become loose, and require to be *frequently* tightened, especially from the forty-fifth to the sixtieth day." The Baron is here in an errour, as to observation. The bandages more frequently become loose, from the fifth to the fifteenth

day, during the rise and decline of the inflammation and swelling; at which time the limb is impatient of all pressure and stricture. "And, after all," the Baron Boyer adds, "with every possible care, there is *generally a greater or less shortening of the limb.*" Boyer, v. ii. p. 86.

We must again invite the attention of the reader, to the author of the "Elements of Surgery." In the first volume, and at page 163, the author most positively asserts that, "By the use of this apparatus, there can be no doubt that permanent extension of the thigh bone, notwithstanding the cavils of Mr. John Bell, may be kept up, and that a perfect cure may in most cases be effected, without the slightest evident deformity, or *shortening* of the limb. I have for twelve or fourteen years, witnessed the effect of this mode of treatment in the Pennsylvania hospital, where more accidents are admitted than in any other institution in America; and I am safe in asserting that the success of the practice has been surpassed by that of no other hospital in the world. I have never seen a *crooked limb*, or a *shortened limb*, the consequence of a simple fracture of the thigh, (neck or shaft) unattended by comminution, where Dessault's apparatus, modified as above, has been fairly applied."

How difficult it is for an author, who writes from his feelings, more than his understanding or observation, to keep on even terms with himself, the reader may determine, after he shall have read the following extract from the same book, page 166: "A *shortening* of the limb *almost always* occurs, more or less perceptible, according as the extremity of the fragments are retained by the capsule; or, as the division being without the cavity,

no resistance is made to their displacement. The muscular action drawing the lower fragment upwards, and the weight of the trunk pushing the pelvis and the superior fragment downward, are the causes of this *shortening*. A slight effort is in general sufficient to remove this shortening, which, however, soon returns when the effort ceases. A tumefaction appears in the arteries and upper part of the thigh, almost always proportioned to its shortening, of which it appears to be the effect."

The reader, no doubt, is anxious to be informed whether this "shortening of the limb, which almost always occurs," took place in the Pennsylvania hospital, where the author, only one moment before, felt himself safe in asserting he had never seen "a crooked, or a *shortened limb*, the consequence of a simple fracture of the thigh," unattended by a comminution, where Dessault's apparatus, modified as above, has been fairly applied ! ! ! Are those facts, or sophistries, or logical arguments, or absurdities? If the reader cannot imagine which, let him consult page 159, of the first volume of the Elements of Surgery, and determine, if possible, whether the author's "whole object was to misrepresent" himself.

In the splint introduced by Hagedown, we can discover none of those provisions which are suited and necessary to meet the objects contemplated in the surgery of a fractured thigh, provided the fracture be oblique, or of the neck of the bone. The knee-splint, of late used in London, is useless, except in tranverse fractures, where no splint can be of any service as regards extension; in such fractures, the ends of the fragments being once placed in proper relation to each other, the very

action of the muscles will retain them, and the bone is, as respects extension, as if it had never suffered a solution of continuity. In the knee-splint, there is no provision against the sinking of the thigh end, in the bed, under the buttock, which even the limb, by its weight, will produce, without the aid of the violent contraction, of the strong muscles, drawing the leg and knee, upon the angle and fore part, or descending plane of the splint.

Whilst the present limited critical account of the fractures of the thigh was in execution, "Mr. Amesbury's new Treatment of Fractures," fell into our hands, and certainly we have in it the pregnant testimony, that what is new is not always valuable—that the most complicated machine, is not the most intelligible in description; and that the sanguine hopes of an inventor, are not, at all times, very forcibly infused into the mind of his reader by his cases of illustration. The first part of the new treatment is made up of a contrast instituted, as tedious as uninteresting, and as uninteresting as unuseful, between the plan of relaxation of the muscles by Pott, and that of extension, by Dessault; the middle part by a description as incomprehensible, as the machine is complicated; and the third, of cases of illustration, the sole effect of which is to persuade the reader that a man without experience, except that of a month or two, is qualified to write, or speak, on the intricate points of a great and extensive practical question.

Of what avail is it to inform the surgical world, at the present day, "that a *simple fracture* of the thigh, *across* the middle," was ultimately managed, by a very complicated machine, so as at the end of six weeks, a man walked without crutch or stick, that a Mrs. Lovel

had the tibia and fibula broke, just above the ancle, and that at the end of six weeks and two days, she was able to walk without a crutch or stick, with two other equally simple fractures of the leg. A student, of six month's reading, would have required an apology for taking up the time of his fellow tyros in such unimportant and uninteresting narrations of success. It is not how to manage a transverse fracture of the thigh of the most simple cast, or to render two or three old women able to walk, at the end of six or seven weeks after ordinary fractures of the tibia and fibula, that the pages of the works of the present period, should be taken up, but in detailing some successful plan by which the oblique fractures of the thigh bone, and fractures of its neck, may be cured without claudication, or shortening.

Mr. Amesbury gravely informs us that he has in store, other cases. We most sincerely hope he will keep them to himself, except they be of a more important cast —we have enough of the "*simple*" sort.

After the experience and practical observation of more than thirty years, we became perfectly dissatisfied with the ordinary machines, whether English or French, and had adopted entirely the plan, recommended by Mr. John Bell, of placing the fractured limb on a mattress, or pretty firm bed, elevating a little, the head, by means of a pillow, so laying cushions along the side, or under the surface of the limb, as to keep it straight, and so that every part of the limb might be equally supported, and, every day, as circumstances might require, extending the limb. Some of the most perfect, as to length and straightness, although the bone, in some instances, was completely comminuted, and crushed, for several

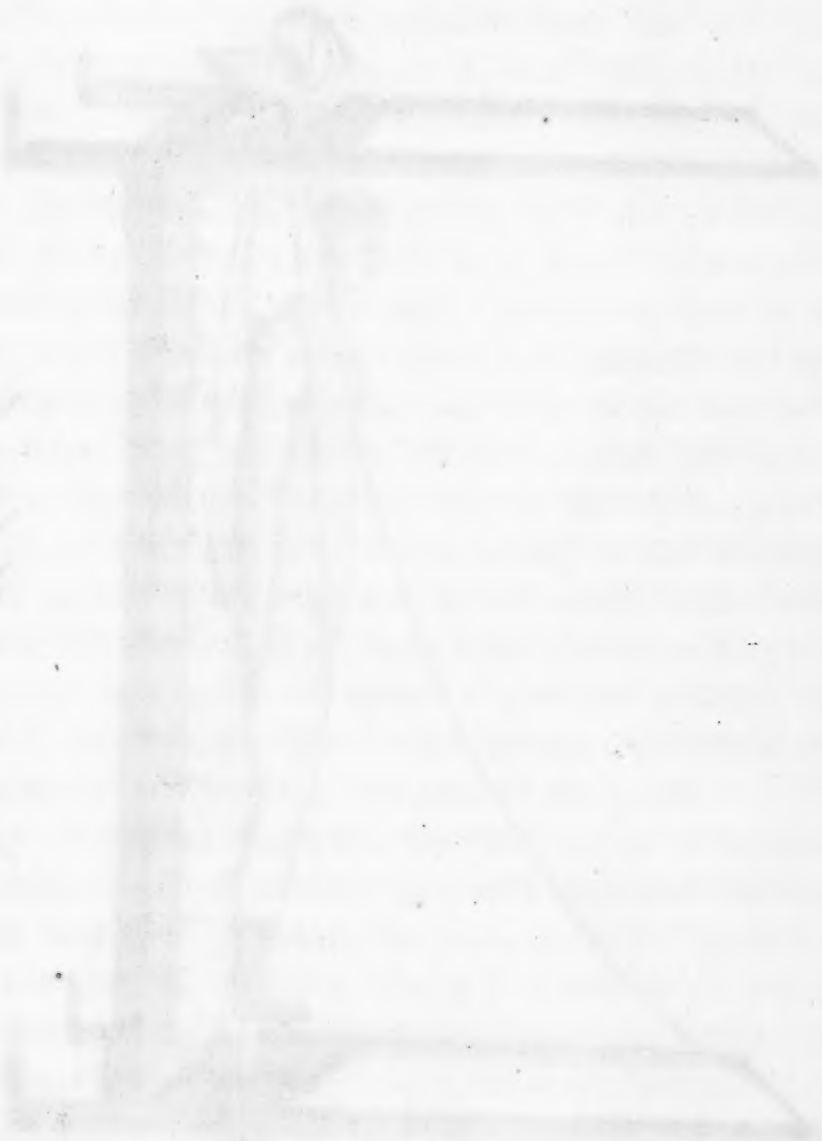
inches by a broad wheel of a loaded wagon passing over it, have been accomplished in this way. Yet, as in common practice, especially in the country, the practitioner cannot pay this particular and unremitting attention, and as the limb can be confined, by gentle extension, to a given degree, without the terrible effects of mortification about the tendo-achillis and anterior parts of the ancle, and the consequences of the pressure on the coverings of the tuberosity of the ischium, evils inseparable from all attempts at "permanent extension," let the pens of writers contradict the result of their practice as they may, we have endeavoured to introduce into common use, a machine suggested, but its form not detailed, in the writings of Mr. Henning. We are uncertain, however, whether the scheme adopted by us, corresponds with the plan had recourse to, by this excellent surgeon. Nor is it of much moment, as to their agreement in form, they are not dissimilar in principle. We abhor professional piracy, and make acknowledgment to Mr. Henning with much readiness and pleasure—our sole object is the promotion of the science, and the benefit of society.

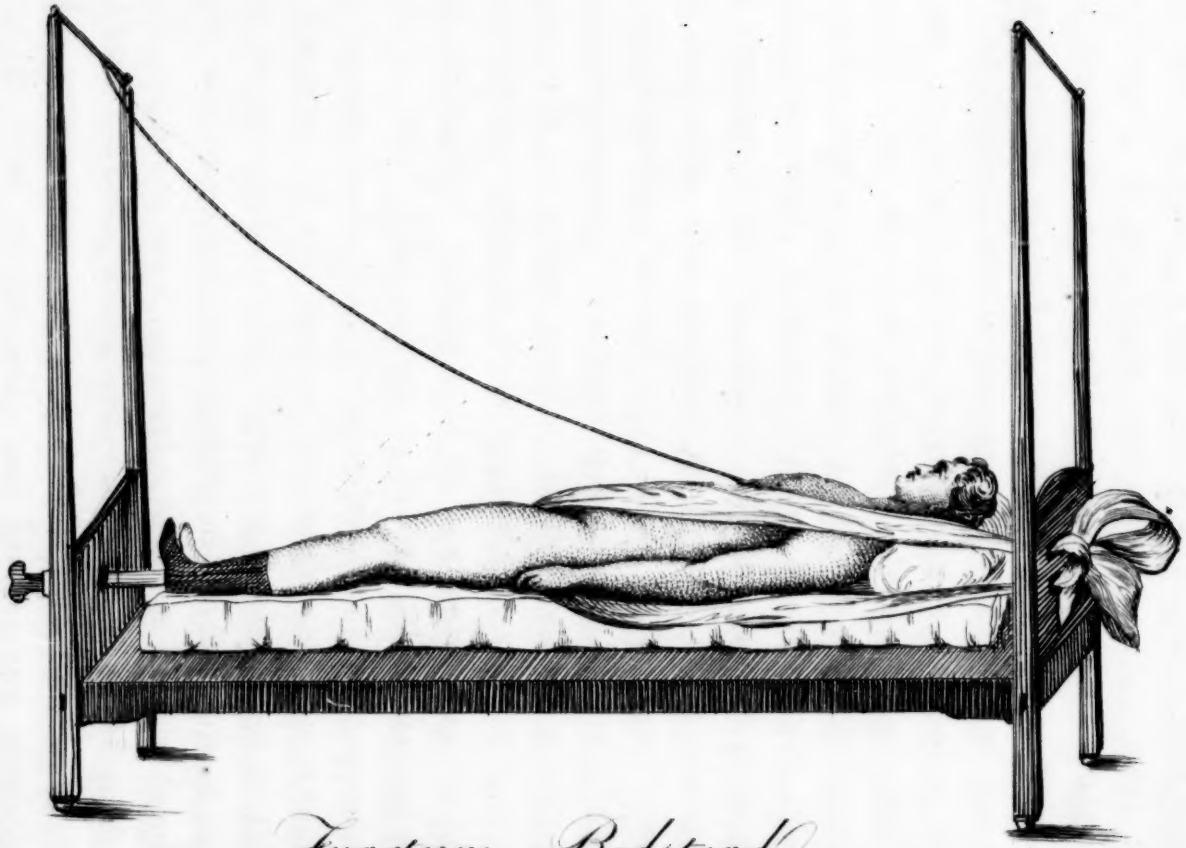
Any bedstead can be converted into a fracture bedstead, by a common workman, in ten minutes—there are merely to be made two mortises in the head board, leaving a bar between them, and two more in the foot board. The former, to permit the two corners of a sheet, laid closely up between the thighs, to pass through, to serve as the band of counter-extension; while those in the foot board admit the strap of the ordinary tourniquet, which is to be passed on through the foot strap of the buckskin gaiter, or chamois band of Gooch and Aitkin, so particularly recommended by Benjamin

Bell in the explanation of his plates, at the back of the volume, on fractures of the thigh.

A common soft sheet, rolled cornerwise, and pressed well up between the thighs, possesses advantages over any belt, or band, which belt or band, as it goes on to be secured to the upper end of the splint, must be oblique in its course. It matters very little whether the band be secured to the head of the shorter splint of the French, which terminated at the spine of the os ilium, or to that of the longer, which reached to the arm pit; its course is oblique, it does not press fairly upon the surface, but obliquely, of the os sedentarium, and of necessity, if tightly tied, or buckled, will affect the circulation on the narrow ridge of the soft parts on which it acts, and produce gangrene. This has uniformly been the result of every effort at complete "permanent extension," we have ever seen attempted. The sheet acts in a line parallel to the axis of the limb, incommodes, when properly applied, but very little, and may be applied equally well on the healthy as on the injured side. This change as to the point, serving as the fulcrum in the counter-extension, is of great moment in fractures of the neck of the thigh bone; in which, from the tumefaction and increased sensitiveness of the groin, and immediate neighbourhood, no fortitude or patience can bear even a light stricture of the oblique band.

The sheet being fixed, one end passing between the body, rather upon its side, and the bed, and the other along in front of the belly and breast, and securely fixed by a firm knot on the outside of the head board, the buckskin gaiter is to be laid round the ankle, so as to embrace well the small of the leg, and press smoothly





Fracture Bedstead

on the ancles, anchillis-tendon, the heel bone, and the instep. The lace which secures the gaiter, or band, may run along down between the outer ancle and the tendo-achillis, or a little in front of the external ancle, and on a soft slip of chamois leather. In this way it will not irritate to any great degree. The ancle band, or gaiter, being carefully braced so as to bear as equally as possible on every point of the surface it covers, the strap of the common tourniquet passed through the upper mortise of the foot board, is to continue on through the strap at the bottom of the foot, and well secured on each side to the ancle band, and return through the lower mortise of the foot board, and thus be secured by the buckle of the tourniquet. Any extension can now be made, that may be thought proper, and kept up at the pleasure of the surgeon, or according to the feelings of the patient, by merely turning the screw of the instrument. In this machine, both bands act according to the axis of the limb; are made of soft and yielding materials, and brought into action by the most efficient and graduated of all forces, the force of the screw.

In laying the limb out, the surgeon is to be careful that every part is equally, and well supported by appropriate cushions—that the cushions are so laid as to assist the distracting force in keeping the limb in a proper direction; the rotation of the limb is prevented by bands or lacs, going from the foot to the two bed posts. By relaxing, or tightening those bands, the foot is supported, and all rotation prevented. These bands, are, in our estimation, preferable to the apparatus used by Aitkin, of the transum and sole. This apparatus of Aitkin for preventing rotation, is somewhat complex; the bands,

on the contrary, are simple; are prepared in a minute or two, and are equally efficient.

To aid the patient in the necessary compliances with the calls of nature, a long band is secured to the foot rail of the tester, and so hangs, as for the patient to take hold on it and raise himself, as circumstances may require. With women, a long flexible gum catheter can be used, so as to take off the urine twice or thrice a day.

In the application of the machine, recommended above, there is nothing of "permanent extension" aimed at. All hopes of permanent extension, are, long since, abandoned by us. And, we are much gratified to find, at least, in our city, that all intelligent practisers coincide in opinion with us, and agree that "permanent extension" is a thing of the imagination, and to be found only where experience or ingenuousness is not present.

During the first days of irritation and swelling, the limb is but gently extended; if thought proper, in strong muscular habits, blood is once or twice taken, as the case may demand, and the bowels are moved, after the first evacuations, not more than once in three or four days; and anodynes, according to circumstances, are called into use. We do not hesitate, day after day, to examine, and move the limb, whether in extending it, or adjusting its course, with freedom. Great attention is paid, from the fifteenth to the thirtieth day, or until the union is effected; which is sometimes within three, and sometimes not until after ten weeks shall have elapsed, yet the union will be ultimately firm and good.

On the thigh, either along its side, or in front, neither splints nor bandages are laid. The limb is left free;

the circulation is without confinement or interruption, and the inflammation comes on and subsides without the slightest pressure or mechanical impediment. All bandages are, in our view, exceptionable, whether the many tailed, Scultets or the roller. They cannot be borne during the rise and progress of the inflammation, which continues for the most part, six or eight days, and during its decline, they are useless because they are loose. Splints bruise and give pain, during the sensitive state of inflammation ; and at no stage, afford the slightest aid.

About two years ago, our services were requested to a boy of eight or ten years of age : his thigh was broken near its middle, and the fracture was transverse.— Knowing that boys of the above age, are to be kept tranquil and quiet by coercion only, we put on the French splint after the manner of Dessault. So untractable, however, was this patient, that he would, in the absence of his attendants, get out of bed with the apparatus dragging at his limb. Finding that neither admonition nor the splint would succeed, we put him on the fracture bedstead ; had a table cloth passed between his legs, and secured to the head board ; then the ankle apparatus fastened ; and, finally, the tourniquet. In this way, we kept him fixed on the bed, notwithstanding the repeated efforts he made to liberate himself, until the limb was completely consolidated. There is, at present, no perceptible difference between the two limbs, and it cannot be discovered, upon the most careful examination, that his limb has ever been broken.

In the latter part of last autumn, an amiable and excellent lady, had the misfortune, by a treacherous step on the ice, to fracture the left thigh bone, somewhat obliquely, and the radius bone of the same side. By the professional gentlemen called in, the fractures were adjusted—to the thigh, the French splint, improperly termed Dessault's, was applied, and remained on about four weeks. At the end of which time, upon examination, the fractured points were found to be loose and in no degree united; the limb was also found to be between two and three inches shorter than the opposite, or well limb; the radius was united.

Upon this unpleasant discovery, we were called in, and after talking over the advantages, at least presumed advantages, of the fracture bedstead, it was introduced, or in other words, the ordinary bedstead, on which the lady reposed, was converted into a fracture bedstead.

As the limb was so much shorter than its fellow, much force was necessary to restore it to its proper length. —This, however, was exerted in a gradual way by tightening the tourniquet more and more, day after day. Finally, the limb was pretty well extended, and, by renewing our attentions daily, it was kept pretty well down. But the reader is not to understand that the extension was regular or permanent. Far from it; on each visit, we had to screw the tourniquet. And, notwithstanding all our attention and care in guarding the points pressed by the buck-skin band, the integuments along down the posterior surface of the tendo-achillis were considerably galled. At the end of about ten weeks, from the introduction of the bedstead, a pretty firm union took place. Long prior to this, an union

had taken place, but certainly not osseous; the limb could be readily bent in any direction, and only by very slow degrees the union became solid and firm. The limb will ultimately be adequate to all the demands of locomotion, but the lady will never move with the same ease and grace she once did; the limb is considerably abbreviated. And the abbreviation of the limb is obviously attributable to the removal of the bands, which, however, was a step, in some degree the dictate of prudence. From the flexible condition of the united points, we conceived it probable that a little irritation might render more active, the progress of union, and give not only a greater volume of ligament, or cartilage, but also increase our chances for bone. In the union of the radius, at the end of three or four weeks, we have an evidence of the competency and disposition of the constitution to form bone. The slowness, therefore, with which osseous matter was supplied in the thigh, must be attributable to the incidental circumstance of the fragments of the thigh-bone not being in proper relation to each other during the first weeks, and something like the first process of the uniting callus having taken place.

Upon the first view of the fracture bedstead, it will be obvious to the experienced professional man, what great resources art possesses in this machine, not only as regards the ordinary fractures, transverse or oblique, of the thigh-bone, but more especially in cases of fractured cervix femoris, wherein, from the contiguity of the injury, and the inflamed and swollen condition of the parts on which the oblique bands must act, if they be applied. In the fracture bedstead, the sheet can press up as well,

and serve as efficiently to prevent the descent of the body, on the well, as on that of the fracture side. And thus, all the inflamed and excitable parts will be free from pressure and irritation. In this, its superiority, over every other machine, is very great; indeed, inconceivable, except to surgeons who have had opportunities of trial.

To give the foregoing memoir additional interest and value, we subjoin the following extract from the

SKETCHES OF THE

MEDICAL SCHOOLS OF PARIS,

BY JOHN CROSS,

Member of the college of surgeons in London; corresponding member of the Societe Medicale D'Emulation of Paris; and late Demonstrator of Anatomy in the University of Dublin.

SKETCHES OF THE

MEDICAL SCHOOLS OF PARIS.

As my object was not merely to see great operations performed, but to look into the internal regulation of the French hospitals, and get acquainted with what is not to be found in books, I used to call at l'Hotel Dieu, at all hours in the day, whenever I passed that way. At the first of these casual visits, a man was brought, who had been hurt by the wheel of a carriage passing over his right thigh. The machine, on which the porters carried him up stairs to the surgical ward, was so well suited to the purpose, as to merit a remark. The half of it, (*a*, *Fig.* 2) against which the head and spine of the patient rested, was so much raised that he was not at all incommoded in being carried up stairs, the spine approaching to a horizontal posture, in proportion as the lower extremities were elevated with the end (*b*) of the machine. One of *les sœurs grises*, or sisters of charity, who act as matrons in several of the hospitals, assisted and directed the nurses in removing the man's clothes; fresh linen was put upon him; all the bed linen was clean; and attention was paid to the patient's comfort, even to the use of the warming pan. I waited above two hours, after seeing him thus comfortably in bed, before the *eleve interne* on guard came to him. There was no apparent shortening of the limb, and *warm aromatic fomentation* was directed to the bruised part of the thigh. The next morning the limb was con-

siderably shortened. Pelletan, at his visit, ascertained that there was a fracture of the neck of the femur, and carefully applied the proper apparatus, according to the method of Dessault.

I saw, whilst in Paris, more reputed fractures of the neck of the femur, than of the body of the bone itself. The French surgeons talk freely about perfect union after fracture of the neck of the femur, as if it would take place, whatever be the situation of the fracture; whilst the common opinion of English surgeons is, that no bony union ever follows the fracture within the capsular ligament. A surgeon of celebrity, to whose kindness I am indebted for seeing many things that would otherwise have eluded my observation, was anxious to convince me of the errour of the English surgeons in this point, and therefore he informed me of the death of a patient in his hospital, that I might not lose the opportunity of being present at the examination. The case, was a woman near sixty years of age, who had suffered a fracture of the neck of the femur, six months before, and had remained in the hospital since the occurrence of the accident, which was, at length, the cause of her death. This surgeon was so sanguine in his belief of the powers of these parts to effect re-union, that he hoped in this case, notwithstanding the great age and weakened state of the patient, to shew me a fracture of the neck of the femur, perfectly consolidated by bony union. On the capsular ligament, which had apparently not been ruptured by the accident, being opened, the head of the bone (*a*, *Fig.* 3) was found separated from the neck, and remained attached to the body only by the ligamentum teres. Not the

least attempt at union had taken place, and the fractured surfaces were as rough as we may conceive them to have been the first hour after the accident. The patient had been kept quiet during the six months that she had suffered from the injury; no motion had been allowed in the limb, and there appeared to have been no absorption of the neck, which is mostly found to have taken place, where the parts have been examined at a remote period from the occurrence of the fracture. In several specimens which I have examined in different museums, whether imperfect union, or no atttempt at union at all, have followed the fracture, this absorption has taken place to a great extent, and in some to so great an extent, that the articulating surface of the bone, which plays in the acetabulum, has rested between the trochanters, consolidated to the body of the bone by ligamentous union, and thickening of the surrounding parts, whilst all the intervening neck of the bone has been absorbed.

I spent half an hour in the museum of *l'Ecole de Medecine*, for the purpose of examining some preparations which the professors produce at their lectures, to shew that bony union may succeed to a fracture of the neck of the femur. I could find only three specimens where union had taken place, and they were all dried bones.

In the *first specimen*, which presented the appearance of (*fig.* 4) the fracture had taken place in the direction (*a*, *b*) beginning at (*a*) decidedly in the neck of bone, and within the capsule, but proceeding obliquely, so as at (*b*) to be quite at the base of the neck of the bone, and external to the capsule.

In the *second specimen* (*fig.* 5) the fracture had taken place from (*a*) to (*b*,) and not merely the whole neck, but also a portion of the great trochanter (*c*) had been separated by the fracture from the body of the bone. The bony union was firm and complete. There was abundant exostosis, as I have endeavoured to represent, and three or four holes in the exostosed portions, big enough to receive the little finger. I believe that all this happened external to the capsule.

In the *third specimen* (*fig.* 6) the fracture on the posterior view was evident from (*a*) to (*b*,) and the bony union was complete; but on the anterior view, there was no appearance of a fracture having existed; so that it seemed as if the fracture had only extended half way through the neck. In the healthy bone such a partial fracture could, perhaps, not take place, but this was evidently a bone from a diseased subject; it was greasy, and at the same time very light (not half the weight of *fig.* 4,) which proved it to be rickety; and the same conclusion might be drawn from the form of the bone.

None of these specimens, therefore, proved to me, that bony union ever follows the fracture of the neck, where the head of the bone, as in the case of (*fig.* 3) becomes insulated, except at its attachment to the pelvis by the ligamentum teres.

There were several examples in the museum of *l'Ecole de Medecine*, where the fracture had actually taken place in this situation, and where no bony union had followed; but as they might possibly be recent cases, where the patients had died soon after the accident, they would prove nothing to those, whose opinions are not already made up on the subject.

When I returned to London, I brought to Mr. Astley Cooper, a dried bone, which M. Roux sent as an example of union of the fractured neck of the femur. I have given a sketch of it in (*fig.* 7 and 8) which will shew that it is such an example as almost every anatomical museum affords, and that it does nothing in determining the pathological point in question between those two eminent surgeons. The fracture had occurred in the direction (*a, b,*) and the great trochanter, as well as the neck, had been separated from the body of the bone. The osseous union, however, in this case was perfect.

The French seem not to have well understood the English surgeons, who state the case, in which re-union by bone never follows, to be where the fracture is entirely within the capsule, and the head of the bone insulated, except at its attachment to the acetabulum by the round ligament.

In advanced age, the neck of the femur becomes very narrow by absorption, and the fracture occurring then is frequently at the narrowest part of the neck, or so as to separate the head from the neck of the bone. The fracture mostly met with in young adults, is the whole of the neck from the body of the bone, including often the great trochanter. A fracture commencing in the neck, and extending to the base of it, or into the body of the bone, forms a third variety. In the two last of these cases, the neck of the bone has connexion with soft parts, is under circumstances common to these, and to other fractures, and perfect union may or may not take place, according to the constitution of the patient, his keeping quiet, or being well treated by the surgeon; but in the first of these cases, where the head of the

femur is under circumstances unlike what are found in any other case of fracture, I have seen no example to convince me of the powers of the system to effect firm and bony union.

Doctor Macartney, of Dublin, has in his excellent Museum a preparation which illustrates well the different powers of the parts to effect union in fracture of the neck of the femur within the capsule, and fracture of the neck from the body of the bone. The subject from which the preparation was taken, was brought into the dissecting room, and therefore nothing is known of the history of the case; it exhibits a double fracture* of the same bone. One fracture is external to the capsule, separating the neck of the bone and the great trochanter, and union has taken place by a sort of ligamento cartilaginous substance. The other is a fracture of the neck within the capsule; here no union has taken place at all,† and the greater part of the separated

* Boyer, in his good work on surgery, has mentioned his having met with this sort of double fracture; but he makes no remark as to the different progress of re-union, within and without the capsule, in these cases. See "Traité des Maladies Chirurgicales." Tome iii. p. 257.

† Notwithstanding what occurred in this case of fracture of the neck of the femur, it is well established that ligamentous union may follow the fracture within the capsule, and bony union the fracture that is external to the capsule. Since I arranged the above observations, I have been fortunate enough, by my acquaintance with Mr. Dalrymple of Norwich, to see a very interesting specimen which he possesses of fracture of the neck of the femur. "A man, seventy-two years of age, suffered this fracture by a fall upon the great tronchanter. After being confined for eleven weeks in the hospital to which he belonged, he died. On dissection, Mr. Dalrymple

head of the bone has been absorbed. This preparation is minutely injected, the capsule is thickened vascular on its inner surface, and containing osseous particles in the outer part of it, which, when accumulated sufficiently, would have formed anchylosis.

I mention the management of simple fractures, as one of the least objectionable parts of the hospital-practice of the French surgeons. They are not anxious to apply early the bandages and apparatus, and they defer doing it so long as there is any swelling or inflammation, for subduing which, they employ warm fomentations or poultices. Their splints are generally clumsy, but great care is given to the padding, and often long narrow bags of linen stuffed with oat-chaff (*balle-d'avoine*) are interposed between the splints and the limb. Their neatness in applying bandages is remarkable, and they never suffer the apparatus to get loose and lose all effect, before they re apply it. This excessive degree of attention, which in cases of compound fractures, would be, perhaps, rightly termed officiousness (because these often do so much better when left quiet and seldom meddled with,) contributes greatly to the

found the capsular ligament entire, and the head of the bone separated from the rest of the neck and insulated, except at its attachment to the pelvis by the *ligamentum teres*. There was no union between the two fractured surfaces; but several distinct bony concretions were attached to the one or the other of them." The bone is preserved dried; three or four of these concretions, arising from the surface of the almost insulated head of the bone, are about the size of peas, and one of them is half as big as a nutmeg; they are brittle white as chalk when cut or broken; and they are very light from the calcareous matter having scarcely any animal substance united with it.

well-doing of simple fractures, which rarely, I should expect, are sent out of the French hospitals badly united.

Dupuytren in fractures of the thigh or leg, sometimes places the limb in a slightly flexed position. In the practice of other surgeons I always saw the fractured limb extended, and the patient lying on his back. I was glad to find at the hospitals, so much attention paid to the rules respecting the position of the limb in fracture of the patella, which writers described well enough, but which I have had opportunities of seeing too much neglected. A woman who had received a fracture of the patella, was brought to *l'Hospice de perfectionnement.* Four or five days after the accident, when the swelling and inflammation about the injured knee had subsided, bandages were applied; and I witnessed the re-application of them at the end of ten days. Previous to undergoing the bandages, the patient was carefully moved into a fresh bed, by placing the head of the bed in which she was lying, against the foot of the other. The beginning of the dressing was, to bandage the leg from the foot up to the head of the tibia, and the thigh, from the groin down nearly to the knee, a part of each of these rollers still remaining unused. Then a piece of linen, divided into three strips at one end, was fastened by the additional part of the roller above the knee, and a similar piece of linen was also fastened by the roller below the knee. Compresses dipt in *warm lotion* were laid on each side of the patella. The treble ends of the two pieces of linen were intermixed with each other, and pulled in opposite directions so as to approximate, or rather to keep approximated, the two portions of the patella. These pieces of linen being

pulled sufficiently tight, were fastened by additional rollers round the leg, and round the thigh. A paste-board splint was put under the knee and calf of the leg; and bags, filled with oat-chaff, were put on each side of the limb, and tied by several tapes, the same as in fracture of the leg. During all this, the patient was kept as much as possible in the position she was afterwards to remain in. The heel was well raised by pillows which supported the whole limb; the trunk was raised by a bed chair; and by means of these two inclined planes, formed by the bed chair and the pillows, the thigh was considerably bent upon the pelvis, so as to keep the rectus femoris relaxed A guard was put over the foot, the same as in other fractures of the extremity.

A more simple apparatus than this will certainly answer quite as well, and is used in some of the hospitals. I have however described the above case thus fully, that I might give an example of the attention of the French surgeons to the proper mechanical position of the body in fractures, a point in which they excel, and to which they attach great importance.

An Examination of the question—Is the human constitution susceptible of the action of the small-pox a second time?

BY CELSUS.

CRESSIA PRODIGIA.

THE human body, in its economy, is fundamentally fixed, and the qualities of all poisons are, in their nature, immutable. Some poisons impress themselves on the principles of life as often as occasion may serve; while others affect the animal constitution but once; of the latter order is the variolous poison. There are exceptions, however, to this general rule, it is stated; there are constitutions, it is contended, that will be acted on a second time by the poison of the small-pox. As the effects of poisons, applied to the human body, are ascertainable by observation only, the subject resolves itself into a matter of experience; for as every poison is generally distinct from every other poison, we could not proceed on analogy—no deduction could be made from the effects of one poison to the effects of another, dissimilar in nature.

That the human constitution is not affected by the poison of the small-pox, as to the repetition or recurrence, as it is by the poison of Lues Venerea, or of yellow fever, is determinable by experience; it is not a matter of simple reasoning; the premises are unequal, and, by consequence. the conclusion could not satis

what would be required by a rigid philosophy. As the poisons are distinct in nature, their effects must vary.

But that a poison could impress its character on the constitution of one individual twice, and not on the constitution two and twenty times—or that a poison could impress its laws on the principles of life in one individual, and not on those of the life of every human individual, is a subject of rational enquiry. It cannot be with any appearance of probability, and I believe is not, assumed, that the principles of the human constitution are fundamentally or in nature different after the second attack, to what they were after the first; nor in one individual to what they are in another. Then if there has occurred no change in the principles of animation, after the second, that are not found after the first, upon what ground can the presumption rest, that there could be two constitutional attacks and not two and twenty? The properties of the poison are the same, and an impression on the body, to be constitutional, must be thorough and full. To say that an impression is constitutional, and yet not complete and fundamental, is absurd and unintelligible. The human economy, to be constitutionally impressed by a poison must be wholly and completely, for a shorter, or longer time, under the dominion of the laws of the poison, not in one part only, but in every part. This, however, is understood to be limited as to degree or intensity of action.

Now if the constitution remain susceptible after one full impression, it must be equally susceptible after a second or a third. The poison could not, by repetition, exert other qualities, or display more than its own

nature. Either the body, then, by the first, was not constitutionally impressed, or if it remained susceptible of a second, it would, according to the rules of all reasoning, remain open to one and twenty attacks. For, by way of contrast, it is well ascertained, that in proportion to the repetition of all poisons, is the diminution of influence and effect. Upon this established law is founded the gradual augmentation of doses in active medicines. A species of reconciliation, as it were, is brought about by the repetition, and in this sort of familiarity or accommodation, is founded the safety of the augmentation. It is otherwise in the effects of mechanical forces. In their application, a certain extent of injury and waste, however small and imperceptible, is produced. And this waste, by repetition, is increased, and finally destructive of the integrity or form, of that against which they are impelled. But were this the case in the effects of poisonous agency on the human body, by the repetition the powers of life would be insensibly depressed, and, eventually, disease and death the consequence. Exactly the converse of what is observed to be the fact. The marble acted on by drops of water, from any given height, continues to be equally subject to detrition and waste, until the whole mass be destroyed. But it is not admitted, even by those who are in opposition, that the human constitution remains equally subject to repetitions of the poison of the small-pox.

The corollary then is, fairly and incontrovertibly, if our reasoning be legitimate, deducible from the above premises, that, if any human constitution be susceptible

of a second constitutional attack. it is equally susceptible of two and twenty, or any unlimited number of attacks.

Further—either the principles of life are radically distinct, in different human constitutions, or every human constitution is liable to be acted on by the same poison, yet not in degree, by which any one human constitution is liable to be acted on. There, undoubtedly, is a difference of liability, as to degree of action, to every poison, in the various constitutions of the human family But this regards intensity; not nature or principle of action.

It is assumed then, as unquestionable, that every human being is susceptible, in its original condition, of the variolous action. If there be a constitution in its original state, and, by principle, not susceptible of the action of the poison of the small pox, that constitution is not human, and the animal to which it belongs, is not man, but some other creature. But it is urged, and in a way, and by authority, entitled to an answer, that human constitutions are occasionally met with, which, however exposed, and under whatever circumstances, continue to be unsusceptible of the variolous impression.

In the first place, we are to be guarded against our disposition, common to all men, to listen to what may be extraordinary in the occurrences of human life; to admire, but not examine. For one account that is apparently true, of this extraordinary unsusceptible character, there are twenty, obviously fabulous. And when the one is stripped of the many adventitious cir umstances that are thrown into the way of our researches, and impede and disable our intellectual efforts, we shall

find but little remaining, to excite admiration, or even gratify credulity.

The difficulties which stand in the way, and obscure our researches, in the affairs and incidents of a course of years, from the cradle to any given epoch of adult life, should call upon our prudence, and render us circumspect in our admissions in respect to matters not subject to demonstration. We need not remark to the intelligent, and medicinally instructed reader, that; as is admitted on all hands, the human body may pass under the variolous fever without any eruption; as every fever, in the constitution of its first paroxysms, is so nearly the same that no science can, during the simply feverous state, determine of what nature the disease may be; and as the small-pox fever, in certain seasons of the year, when the atmosphere is charged with the marsh poison, would exhibit something of the character, for the first few days, of the remittent, or intermittent form, and nothing but the eruption could determine the nature of the disease, it is impossible to ascertain that those constitutions have never, in fact, been susceptible of the variolous action, and have not really passed under its impression. That the disease was never present, and that it did not display its phenomena in full and indisputable characters, are separate and distinct points. The latter, the formal display of eruption, is not necessarily consequential on the former, the feverous condition.

Again; the researches and actual observations of the profession have settled it as a fact, that the child, in utero, is occasionally affected by the poison of the small-pox, during the action of the poison, on the

mother's constitution. And it is not impossible, but a child, in utero, may be affected, although the mother may have previously had the disease, if she continue any sufficient time where the poison is present. The poison might possibly affect the unborn child, and yet not sensibly discover itself in the mother. But we may be turned about, and told that this, our speculation, is more specious than solid; experience is against us. We will examine this objection.

Experience, to be authority, presupposes three conditions, and if it have not those to enter into its constitution, we are excusable in laying it aside. Experience, to be authority, then gives the assurance to the understanding of the reader; first, of ample opportunity to collect materials: secondly, of ability to use those materials: and thirdly, of integrity, faithfully, to report the result of observation. As regards the present question, we must rigidly insist on those three requisites; we cannot consent to yield any point of our ground to assertion, merely because it is positive or dogmatical; nor do we wish the reader to admit any thing to which his good sense is opposed, on our part.

The small-pox, is a Saracen disease; was by that people introduced into Europe about the sixth or seventh century, according to the account of the learned Dr. Freind, and not so late as the period of the Crusades, as has been insisted on by Le Clerc, and others. This chronological circumstance, however, is of little moment in our present discussion. So far back, as the authority of history, at all reputable extends, the disease was propagated among the Saracens—the Spaniards—and the Welsh, by inoculation, or what among them, was

generally termed, "the buying of inoculation." For it appears, their superstition insinuated, that except some price were paid for the matter, it would be inefficient. A popular usage, of whatever nature, is for the most part, the result of experience, and the usage of inoculation, could not have been among rude nations, derived from speculation or science; it could have grown out of observation only. We hold ourselves justifiable in the inference, that the historical fact of inoculation being customary among those uncivilized and unlettered nations or tribes, is strong presumptive evidence that the individuals who had suffered under one constitutional attack of the small pox, was not liable to a second. For what could have led to the factitious method, if the disease in the usual way, would occur a second and third time? The understanding of each individual, and, by consequence, of the nation, must have apprehended that a disease which could occur twice, or more frequently, in the same individual, might, under favourable circumstances, occur in any or every individual. Nothing, to our understanding, could have induced rude and uncultivated nations, to introduce, by art, a disease, which must have been, in a vast multitude of instances, fatal, except the important and uniform fact, that this disease did not attack a second time. In the first place, it could, by experience only, be ascertained that the disease induced by art, would be the same, as that by natnre. And indeed such a question has arisen, and on it the world has been amused by disputes, as keen and warm, as unphilosophick, and ridiculous.

A parent might be induced, where it was uniformly observed, that a violent and dangerous disease attacked

but once through life, to subject his child to an experiment, under such circumstances as would allow of parental attentions. This would recommend itself, on the score; that the child would, pretty certainly, at some future period, be liable to the disease; that its chance of getting through in safety, and without deformity, would be much better, under the tender attentions of a parent, than if exposed to the indifference and neglect of strangers. On those two grounds only, we presume, could inoculation ever have been introduced among any people. We therefore urge, that this early and general usage of inoculation among uncultivated savages, is strong, although presumptive, evidence that the disease was not observed to occur a second time in the same individual. It is certain, that nothing short of such a conviction would have induced the civilized and enlightened nations of Europe, in modern times, to have had recourse to inoculation; and that too, under the most guarded and circumspect provision against accident and mischief. The reader can look at the history of the introduction of inoculation into England, France, Germany, &c. The common sense of mankind is the same at all periods and among all people.

It is asserted by some authorities, that the Arabians did admit of an occasional recurrence of the disease in the same subject. But it is probable that in this, there is some mistake. The Arabians admitted, that although the small-pox should attack in some of its modifications, yet the patient would be liable to the confluent variety. To explain this, but a few words are necessary. The Arabians believed the chicken pox and measles to be modifications of the small-pox. Should the

person, then, after having had either of these, viz : the chicken-pox or measles, be exposed to the confluent variety, he would receive it. There is no fact, however, that we can find, to the period of Boarhave, of any person, who has ever laboured under the *confluent small-pox*, being susceptible of a second impression. In confirmation of this, the testimony of Boarhave is very strong, and very pertinent. He says "that the *distinct* small-pox, did not invariably secure the individual from a subsequent attack of the *confluent* form, though the *latter* effectually prevented a recurrence."

From the very general practice of inoculation among the Arabians, the Spaniards, and the Welsh, about the seventh century, and afterwards, we are justified in concluding that the matter for inoculation would be taken from that form of the disease by which the body would be guarantied, and not from the more equivocal and uncertain form which would still leave the body liable to a second attack. For why would they artificially give a modification of disease which was observed to be followed, in many instances, by a worse?

Until the days of Heberden, it was not distinctly understood, that the chicken-pox is not a modification of the small-pox, and hence, the frequent errours as regards the recurrence of the small-pox a second time, in the same individual, in the writers prior to his time.

On the authority, then, of this great and justly celebrated physician, (Boarhave,) surely one of the strongest and fairest authorities of any period, whether as to opportunity to collect, ability to use the materials, or integrity to report the result of observation, we are authorised in the conclusion, that the natural or confluent small-pox,

did not occur a second time, in the same individual, and if there were cases of constitutional recurrence under inoculation, or what we may term the distinct form, one of two things must be admitted; either the small-pox by inoculation, or in the distinct form must in its *nature* be different from the small pox when casually, or in the confluent way, introduced; or there was some illusion in the circumstances of inoculation of the distinct small-pox There is no alternative; either there is a difference as to nature, or as to circumstance.

The observation and experience of the profession, render it impossible for us to admit any difference as to nature. By almost innumerable experiments, (in taking matter from the natural pustule, the mildest form is produced, and in taking matter from a pustle of the mildest form, under inoculation, the worst form of the confluent is, at times, the result,) it is settled that in nature the disease is the same. The difference, of consequence, must regard the circumstance.

By the presence of some morbid action, or peculiar circumstance of the constitution, at the time, the matter inserted by the point of the lancet in inoculating, produces a local disease only. For although the pustule will rise with all the form and character of a legitimate pustule, the general habit will not be influenced. In such a case, where the pustule is regular in all its character; where matter is taken from the pustule, and, with this matter, other individuals are successfully inoculated, there can be no doubt as to the genuineness of the local affection, but if the constitution does not participate, the disease does not constitutionally exist, and the body, upon a suitable occasion, will be affected.

This, however, cannot be deemed a second constitutional affection, or the occurrence a second time of the disease in the same habit.

Either there was an illusion as to the circumstances of the distinct small-pox, or the chicken pox must have been mistaken for the distinct small-pox. A thing very probable, and almost inevitable, at a period when the small pox and chicken-pox were conceived to be mere modifications of each other; in fact, the same disease, under various forms. These mistakes gave a kind of tincture to the medical writings on this head, for years. And until very lately, much doubt was entertained whether the confluent small pox and the inoculated small-pox be the same; a dispute finally settled by numerous and laborious experiments.

In this conclusion and reasoning, we are strongly fortified by the authority of the learned Mead, who assures us that, "as to those, who, after having been inoculated with success, (constitutionally,) are, notwithstanding this, said to have suffered the small-pox, I must protest, that, after the most diligent inquiry, I have not been able to find out *one* convincing proof of this kind."

The same conclusion is sustained by the present Dr. Bailey, although but indirectly. On the general subject of certain diseases affecting the human constitution but once during life, this diligent and observing physician, is not competent, from his experience, certainly, sufficiently ample as it was laid out in the extensive field of a London practice, to furnish a solitary instance of the second occurrence of the small-pox in the same person.

In proof that he was not indifferent to the discussion of the interesting subject, he says that he has met with one or two instances of the second occurrence of the measles in the same habit. Of this observation, in respect to the measles, we shall presently take notice, after having introduced the authority of Dr. Heberden. Could the observation of Baily, respecting the recurrence of the measles, in the same constitution, be supported, we should consider it nearly tantamount to evidence, in favour of a second occurrence of the small-pox, so far as the authority of this learned physician could go in determining a point scarcely to be settled by any authority under particular circumstances.

"Scrupulus autem aliquis mihi restat de securitate eorum, in quibus pustulæ vel nullæ eruperint, vel nunquam rite maturuerint, licet in loco inoculato ulcus factum sit cutem erodens, et non nisi post multos dies sanatum. Vidi enim fœminam variolis numerosis occupatam, quæ solito more suppuraverunt, et exaruerunt; quamvis mihi fidem fecit se ante annos decem fuisse inoculatam, et post quinque dies febricitasse cum quodam capitis dolore, *quem secutæ sunt pustulæ paucæ, cito quidem recedentes sine suppuratione;* at pars inoculata alte exulcerata fuerat per decem dies, et dein lata cicatrice notata, quam ipse vidi." Comment. Guliel. Heberden, p. 378.

"But yet I have some doubt as regards the security of those in whom either no pustules appear, or where they do appear, but do not fill, and mature, although the inoculated spot destroy the skin, and does not heal for many days. For I saw a woman, labouring under a numerous eruption, which suppurated, and desquamated, in the

usual manner; although she satisfied me, that, ten years before, she was inoculated, became feverish, with some pain of the head, which was followed by a *few pustules*, quickly disappearing, without suppuration; but the inoculated part was deeply ulcerated for ten days, and left a broad cicatrix, which I saw."

Heberden, among the most finished and elegant scholars, accomplished and learned physicians, of which the British history makes mention, who lived to a very advanced age, was in the regular habit, as is seen in his commentaries, of recording all that he deemed valuable in the course of his observations, which were laid out on the extensive scale, perhaps, of the first practice of London, and continued to a very advanced period, in the extract above, gives the most ample and satisfactory evidence that he never witnessed a second occurrence of the small-pox in the same habit; that he had, however, met with an instance of a woman, who had, under his attention, unequivocally the constitutional small-pox, but who gave him reason to believe that, ten years before, she was inoculated with the matter of small-pox—that her arm was sore, leaving a broad cicatrix, which Hiberden saw—that the pustules were few, and passed off quickly without suppuration. It is apparent, and undoubted, that Heberden did not consider the first case as one of a constitutional character, and, in the plainest terms, he puts the reader upon his guard, by the expressive terms, "*pustulæ* vel *nullæ* eruperint, vel *nunquam rite maturuerint, licet in loco inoculato ulcus factum sit cutem erodens et non nisi post multos dies sanatum*." It is as much as if he had said, "although there may, or may not, be a few pus-

tules, which do not mature, and an ulcerated arm continuing for many days, yet the case is not to be considered as constitutional But the inference is; if the pustules rise, and fill, and maturate, in a legitimate (rite) manner, then is the disease constitutional, and of the security of the patient, I have no doubt.

And, immediately upon his opinion, by way of illustration, he furnishes an example, in a woman who had been inoculated ten years previously; had fever; a few pustules, which quickly disappeared, without arriving to maturity, &c. but who, upon exposure to the poison, took the small-pox, and under his attentions, passed through it in its regular and legitimate constitutional form.

If a life, a practice, and an assiduity, such as that of Heberden, do not furnish one solitary unequivocal case of a second occurrence of small-pox, in the same subject, how modestly and carefully should men speak who are scarcely fledged with the earliest down of experience.

We have placed a few of the most distinguished authorities in medical literature, before the reader, and could add a multitude of others, were it necessary and proper to extend the catalogue in our own favour. Our opponents, and, perhaps, the reader, would be better satisfied, that we should pay attention to what is opposed to us, and show upon what ground the authorities are to be rejected that sustain the opinion of a second occurrence of the small-pox, in the same person.

We put our examination upon the following grounds, as suggested at the commencement of our discussion:

1st. The opportunity of observation, and collection of facts.

2d. The ability to make use of the facts when collected.

3d. The moral fidelity to report, deduced from the general standing of the person, and his professional reputation.

4th. The difficulty of discriminating between the small-pox and the chicken pox.

5th. The caution with which general medical reports are to be admitted.

The inoculation for the small-pox, during the life of De Haen, was not general; so far from it, restrictive laws were enacted, and penalties imposed; by consequence, De Haen could have but very limited opportunities of becoming acquainted with the phenomena, in their ameliorated form, under the management of art, or of their near approach to those of chicken-pox. Added to this, he was among the most intemperate in his zeal to enlist the passions and the prepossessions of the fanaticks of the day, against the introduction of inoculation, and contributed in no small degree to engage "the enlightened nations of Europe," to suppress inoculation, upon assurances that even should inoculation succeed, it would be a tempting of God, and be followed or attended by the curses of heaven.

Assuredly, under the circumstances of an unimproved profession, as regarded the distinctive signs of small-pox and chicken-pox, and the influences of a strong superstitious frenzy, De Haen cannot be quoted as among the respectable, or even creditable authorities, on a subject so interesting to science, and involved in so much obscurity. However masculine and capacious the mind of De Haen may have been, limited in observation,

and prejudiced as he was, he could have been but little conversant with the mingling shades of the lighter small-pox and severer chicken-pox, even had the distinction between these two generick diseases been recognized and established at his time. A prudent and modest science, aided as it is by our present stock of experience, would, with great circumspection, undertake, without experiment, with the matter on a healthy body, to determine between the equivocal phenomena of the above mentioned diseases. Against the authority of De Haen, on every score, we must protest.

As our attention, has, of late, been called to the subject, and as it is not wholly irrelative, we will offer a few remarks on the warm recommendation, and urgent instance of De Haen and his coadjutors, to "the enlightened nations of Europe," who, under their admonitions, had their parliaments to pass the most heavy enacts, threatening, "with the severest penalties," such physicians as should dare to think as physicians, and, taking observation and fact as their guide, should prefer the inoculated to the casual manner of propagating the small-pox. Not only did those *learned* and *pious* parliaments most ignorantly and impiously menace with the severest penalties, or the punishment due to mis-misdemeanor the physicians who should take the work of disease and death out of the hands of the Almighty, but most wickedly transmitted their proceedings, and we might almost presume their intemperate feelings, as precedents to succeeding ages.

It would be well to explain upon what ground these deliberative bodies undertake to give form and direction to the high destinies of the profession of physick. Is it

because they are more learned in a science, the most remote from their studies and their views? that they are more patriotick? or that they are more virtuous, than physicians? Or is it from a mere inflation of vanity inspired by the complimentary addresses of those who play on weakness to inflame passion, or make instruments of force to silence the voice of argument, and suppress the commanding energies of reason? Leave reason and argument free, and communities are safe—professions will take care of themselves.

To whom are the immense losses of humanity—the deformities and deaths, which followed in the train of those barbarous interpositions, to be referred? To the parliaments, those ignorant instruments of selfish or wicked designs, who, by the volumes of their enacts, smothered the voice of instructed medicine, or to the *virtuous* and *enlightened* men, who advised and recommended the measures?

The parliaments, however, elated by the compliments of De Haen, and such as De Haen, were not restricted and satisfied, by imposing deformity, misery, and death on society, by their decrees in regard to the small-pox, but made further advances, as is customary with power, unchecked by knowledge or virtue; the parliament of Portugal, made it capital for any physician to bleed in the wrong arm, in cases of pleurisy, and that of France, arbitrated the merit of individual physicians; medals were struck to the honour and credit of Sigault, who had the address, to persuade Chaptal and his colleagues in the commission, to make their splendid report to the academy of Paris, on the affair of the sectio symphysis pubis; in which it was

subsequently confessed by Chaptal, there was not one word of truth. Read, on this subject, Baudeloque and Louverjat.

And it is not long since a parliament of one of the enlightened nations of Europe, that of France, debased the honourable and liberal profession of surgery, to an union with the low and ignoble trade of the razor. Upon which, every member of the profession, of liberal education and honourable views, became a private gentleman, by the abandonment of his profession, and it is confidently expected, that, whenever Legislatures and Parliaments, shall interpose their authority, in giving direction and form, to a profession certainly second to none in learning, virtue, or utility—every physician, who possesses the delicate sentiments of a gentleman, and exalted feelings of his rank, will retire and leave those, whose modesty may suggest the advantages of the co-operation of Legislative bodies, to whisper lessons to instruments, as insensible to the dignity, as ignorant of the importance of our profession. How gratifying indeed, to the learning and pride of the science of physick, must be the late admonitions contained in the Medical Recorder ! ! How irreconcilable are those admonitions, with the following opinion, extracted from the speeches of Lord Erkine ! "What ? ——, but they are to be let in (called on) for the purpose of governing one of the wisest, and the most learned bodies ; of governing men, who one and all of them, almost, are deeply skilled in every thing, that learning and science have brought forward in any age."

De Haen relates an instance of a student of law, "in whom the small pox had occurred twice. The first

attack left him pitted, and the second proved fatal." Now as it is admitted, by every physician of reading, that until the days of Heberden, about fifty years ago, when he wrote on the subject, the distinction, between the phenomena of the small-pox, and chicken-pox, was not recognized, and that the distinction was first settled and determined by Heberden, the reader will experience no difficulty, in what manner, wiser and better informed men, than De Haen, might have been imposed on.

"One of the most striking cases of a second attack is that of Mr. Langford, whose countenance was remarkably pitted and scarred." Memoirs Med. Soc. London.

Of the many thousand cases of countenances pitted and scarred by small-pox, this is the only one that is brought forward in evidence of a second attack. To our understanding, this solitary case, instead of being a striking instance of a second attack of small-pox, is, in its solitariness, a strong circumstance in favour of some misapprehension. If there were no source or sources whence such scarring and pitting might be derived, then, certainly the evidence furnished by it, would be striking. But as there are sources, which, by the most fastidious philosophy, might be admitted, we are justified in putting a different interpretation on the fact.

It is, in the first place, too remarkable, that there should be only one case in all the annals or traditions, true and untrue, gathered from the experience of all Europe, of a scarred and pitted countenance, from small pox, being subject to a return of small pox. It is in our opinion much more probable that this insulated

case, of a scarred and pitted countenance, is derived from some other sources ; sources probable and admissible. The Crusta Lactea of childhood would, where the child through fretfulness had been provoked to scratch and wound his face, give all the scarring and seaming. The same condition might result from a burn—and either, to an intense degree, succeeded by a severe case of chicken-pox, especially in a scrofulous habit, might produce a character of countenance, in every respect analogous to that of Mr. Langford. That Mr. Langford's countenance was scarred and pitted, afforded no evidence, that both or either, was by the small-pox; the one, the seaming, may have been the production of the Crusta Lactea, or a severe burn, and the other, the pitting, the result of the chicken-pox. There is nothing in the pit of the small-pox, to distinguish it from the pit of the chicken-pox, and it would be very difficult to discriminate between the seaming and scarring of a burn, and what are produced by a small-pox. We must insist that if after so severe a case of confluent small-pox as Mr. Langford's, is imagined to be, a second attack would be made, that his case could not be solitary. The simple circumstance, as already remarked, of its solitariness or oneness, is a strong presumptive evidence, almost conclusive evidence, indeed, against it.

The case referred to by Dr. Laird, is too bald, and too much without circumstance, to be entitled to any serious consideration. To us it is only proof, that a man of sense may be sometimes in errour, when he permits his imagination to outstrip his discretion.

As to the sixty or seventy cases brought forward by the sedulous efforts of Dr. King, we must be indulged with the question; whence and from whom were they procured? It is somewhat extraordinary, that no reference is made to the name of Fothergill, of Lettsom, of Warren, of Reynolds, of Heberden, of Baker, of Sim, of Baillie, &c. &c. They almost held dominion over the immense medical field of London. They surely were competent to observe, prudent to discriminate, and virtuous to report. Perhaps they were too competent to observe, too prudent to be imposed on, and too virtuous to attempt to play off an imposition on others. It does sometimes, it is true, fall to the lot of the subordinate men to make discoveries, but such discoveries are for the most part solitary, and like angel-visits, "few and long between."

For the medical learning and understanding of London, we feel a profound veneration. We, however, discover among the fine collection of pure metals, which throw a lustre over the medical world, not a small mass of alloy and gross ore. When contemplating the great city of London, our minds do not unfrequently stray back to ancient Bagdad, whether we be led by some professional affinities, or some less apparent instrumentality entice us, we will not determine, but such is the fact. Of Bagdad it may be said, that all her physicians were doctors, but all her eight *hundred doctors* were not physicians. Would this motto be out of place on the monument of the great city of England? In which there are upwards of one thousand medical men, but about one hundred and sixty physicians only.

When cases are hereafter given to the world, which call for more faith than a decent respect for our understanding has to spare, it is proper that name and circumstance will be furnished. It is treating the common sense of the world, with too much disrespect, to have its credulity and more generous feelings, laid under perpetual contribution. In a late number of the Medical Recorder, we have perused a long and copious account of instances, where, according to reports from surgeon's mates or nurses, or probably from those less informed than either, the small-pox has occurred a second time, in the same person. The zeal and labour of Dr. Mitchell would be held in much higher estimation, had he displayed his excellent talents in settling the sensible phenomena, or criteria, by which, at all times, and with certainty, the globated variety of chicken pox, could be discriminated from the milder instances of the small-pox. The Doctor must be aware, that, by the most able and experienced practisers in the profession, it is conceded, the shades and characters of the milder small-pox, and severer chicken-pox, so intermingle, and mutually run into each other, as to leave no interval on which the best instructed judgment can act, except by experiments with the virus of each, on healthy and susceptible habits. The professional world is already too much encumbered with medical cases and reports, and the pen of a respectable writer might, in our opinion, have been better employed than in disseminating notions, too crude for science, and too improbable even for generous credulity.

Were we to give, in detail, a catalogue of the wondrous and extraordinary things real, and mockeries

unreal, of which we have heard and read, the patience of the reader would fail, and his understanding be overwhelmed. We have read of a child found in the abdomen of a boy, in France. It is true the wonderful history was not accompanied by any description of testes and ovaria, found in the belly of this boy, nor any probable conjecture, as to what other organs might have performed the acts of procreation, or whether this child was put into the belly of the boy without an act of procreation. We have also read of children born in good condition, without navel strings, or with their navel strings rotten and mortified; some, where the navel was completely cicatrized over, and yet the health of the children, vigorous and robust. The reader can consult Monro's works. Moreover, an opportunity has been furnished, while looking over the celebrated essay of the learned Rush on the spleen and thyroid gland, to observe the slender utility of the spleen, in the human subject. Upon the removal, whether by *disease* or *art*, (the surgeon's knife) of this large viscus, the eloquent writer assures us, that very little more will ensue than a slight change in the secretion of the saliva, semen, &c. ! ! Added to what we have already narrated, in Dr. Dewees' Essay, the naturalist and the physician, may learn that painless labours are so usual and customary in Philadelphia, that to record them would be rather a work of supererogation. Our stock of the wonderful is by no means exhausted; but, perhaps, our detail has extended far enough, for the present, of those prodigies which certainly can never occupy any part of the page of instructive history, we therefore will return to the subject of Dr. Baillie's discovery of the

"several instances of the measles occurring a second time in the same individual."

Preliminary to what we may say on the authority of so respectable a physician, as the author of the "Morbid Anatomy," it is proper to enquire whether there be any disease, which, in its sensible forms, so nearly resembles the measles, as under certain circumstances, cannot be discriminated from it, except by experiments made by the effluvia, on healthy habits.

Of what is, by Sauvage, termed the Uredo, by some writers the Febris Urticata, by others, the Scarlatina Urticata, in the nosology of the editor of the Baltimore Journal, the Rubecella, and by himself, the Urticaria, Cullen gives the following definition.

"Die Secundo," he should have added, vel tertio, "rubores maculosi, urticarum puncturas referentes, interdiu fere evanescentes, vespere cum febre redeuntes, et post paucos dies in squamulas minutissimas penitus abeuntes." Nos. p. 148.

In the course of our professional life we have had frequent opportunities of observing this disease. And we have uniformly discovered, in addition to the above, that the following symptoms have manifested themselves, viz : the vox rauca, tussis levis et sicca.

The Urticata, Uredo, or Rubecella, as may suit the taste of the reader, is distinguished by all the sensible signs of the Rubiola, except the sneezing, and weeping eye. And, as far as our reading and frequent observations enable us to form an opinion, is distinguishable from the ordinary measles by such almost imperceptible shades as to render it impossible for a Bailey, or any other physician, however trained in the school of expe-

rience, to form a positive and satisfactory opinion at all times.

We value the authority of Baillie, but, have seen too much on this subject, to allow even to his descriminating mind, the distinction of authority, except, as we have remarked, where individual cases have been illustrated by experiment on the healthy constitution with the effluvia thrown off during the morbid action; insulated cases are too equivocal.

In the opinion that the Urticaria of Cullen has been mistaken for the real Rubiola, we are strongly fortified by the striking circumstance of the measles never returning more than the second time. Why not a third, and a fourth time? We answer, simply, because there is but one disease that can be confounded with the measles, viz: the Urticata. When Dr. Baillie can give us the criteria by which the Rubiola and Urticata, can be uniformly distinguished, we will respect his authority.

To put an end to our present limited and imperfect remarks on the small pox, occurring a second time in the same person, we promise, whenever a case can be fairly made out, of a recurrence of this disease in the same individual, we will give up our opposition, and fall into the general opinion of medical speculators. Our opponent, must, however, lay down, in an intelligible form, the distinctive marks, by which the small-pox, in all its modifications, and the chicken-pox, under all its varieties, can, with certainty, be mutually discriminated—the case must have the authority of a name adequate for intelligence; amply provided with opportunities to collate premises, and deduce conclusion; and credita-

ble for that personal standing which throws a lustre over all intellectual distinction and acquirement—we mean moral dignity. We are not to be put off by assertions, and shall take the liberty to analyze all stories, or fabricated tales, from whatever pen they may emanate, with the utmost scrupulosity and rigour.

EXTIRPATION OF THE PAROTID GLAND.

BY

JOHN B. DAVIDGE, A. M., M. D.

TENTANDA VIA EST.

THE parotid gland, from its particular relations to, and connexions with, important parts, has seldom been the subject of operative surgery. And although we are furnished with various methods of removing scirrhous glands, in most other parts of the body, yet we seldom meet with directions for the extirpation of the parotid. This gland is frequently indurated, and enlarged to a monstrous size, and requires much care and attention in its removal, as it is connected with the trunk of the external carotid; some of its branches, and the partio dura of the seventh pair of nerves. Many surgeons of reputation assert the removal of this gland to be impossible, and, if attempted, fatal to the patient.

It is readily acknowledged, that an attempt at extirpation ought not to be made, except by an operator, whose anatomical knowledge is accurate, and who can trust his own hand; but the assertion that the parotid cannot be extipated, is rather unqualified; an assertion which might find an apology with the inexperienced and the unread.

The trunk of the extreme carotid, which passes through the substance of the gland, renders an operation, it is true, somewhat hazardous. Both the trunk, however, and its branches, can be commanded and secured by ligature. And even were it necessary, under extra-

ordinary circumstances, the common carotid itself, can be had access to, and by a previous application of a ligature on it, all hemorrhage from the branches deriving their origin from this great trunk, could be controlled.

The parotid gland was several times a subject of surgery, with Heister, from whom much of what I shall say on the subject is taken, and, he assures us, without the loss of a single patient.—Surgery, when this able physician practised, was by no means in a state so adequate as it now is, for such serious undertaking, yet a skilful management of the bandage, and other measures, enabled the bold and the dexterous to succeed. At present, surgery is enlarged in its borders ; its views are more extensive ; and the aid it derives from anatomy, far more valuable and certain : especially the modern practice, probably derived from the French, of securing, by ligature, great and important arteries before they are divided.

Perhaps it may be proper, if not necessary, before I proceed further, in regard to the operation, to say something of the anatomy of the gland, and its immediate relations. The surgery of no part of the body, more particularly demands accuracy in anatomical knowledge, than that involved in the extirpation of the parotid gland. The keen contest among surgeons, can by this accuracy of knowledge alone be settled, as regards the practicability of removing this gland by art. If the extirpation be impracticable, the reason of such impracticability must be looked for in its anatomical connexions. If there be nothing in the anatomical structure which forbids the use of the knife, then some regard is due to those who assert that the gland is removeable by surge-

ry; and, on the contrary, if the anatomy wholly interdicts the use of the knife, all expectation must be abandoned.—I will lay before the reader a few authorities.

"The parotides are two large, whitish glands, irregularly oblong, and protuberant, situated on each side, between the external ear, and the posterior ascending ramus of the lower jaw, and lying on some part of the masseter muscle. The superior portion of this gland lies before the cartilaginous meatus of the ear, and touches the apophysis zygomatica of the os tamporis, and it is extended forward and backward, under the lobe of the ear, as far as the mastoid apophysis."

"From the anterior and superior portion of this gland, a white membranous duct, or canal, is produced, by the union of a great number of small tubes, representing so many roots," &c. "The external carotid artery and vein, and the potio dura of the seventh pair of nerves, pass through the substance of the parotid gland, to which they give branches; and the facial artery and vein pass over its duct."—System of Anatomy, v. ii. p. 185, Edinburgh.

In the above description, taken from the first authority of Britain, the anatomical surgeon can perceive nothing that should render the extirpation of the parotid gland impracticable. The division of the external carotid, a little after the bifurcation of the common carotid, into the internal and external, or any of its branches, in no degree implicates the life or future comfort of the patient. The portio dura of the seventh pair of nerves must, also, be divided, both before it enters the gland and again, a little after it emerges from the gland. The result of this division, however, in two places, will be

unimportant; the muscles, to which the nerve is destined, will, by this division, be in some degree palsied, and of consequence an obliquity, of greater or less extent, of the face be produced; but this is by no means so serious an evil, as to constitute a solid argument against the removal of a disease, which, if not eradicated, would in the end prove fatal. After some time the muscles, will in some degree, recover their wonted sensibility, yet never perfectly.

This partial recovery of their sensibility by the muscles, has been a subject of warm and serious debate; some have referred it to a reunion of the nerve, but this could not be the fact, when, as was done by Mr. Abernethy, a considerable portion of the nerve was removed. Under such circumstances, either the fine nervous filiments must communicate at their points, which is by some anatomists, said to be demonstrable—this however is questioned by high authority—or each nerve, and portion of a nerve, must, as so much brain, possess the faculty to renew its own power, and by an energy intrinsick to itself, recover from any incidental injury, or dilapidation. The latter of these, is, in my opinion, the more probable of the two hypothesis.

That a nervous ganglion is so much brain, as to influence and energy, is pretty generally admitted, and that a nerve, according to its volume, is equally possessed of cerebral capacities, may be inferred from numerous experiments on nerves; especially the experiments of Wilson Philips, in his Enquiry, where he proved a thing very different to what he intended to prove; he established, to our understanding, that, by the influence of galvanism, the stomach, after its nerves were divided,

could be enabled to digest aliment put into it. It is true, his inference from the experiments, is very different. He concluded that the galvanick power is the same as the nervous, although he acknowledges that by urging the experiments, and repeatedly charging, with the galvanick influence, the parts became highly inflamed. Would any degree of nervous power, in a stomach, the nerves of which had not been divided, or even if they had, inflame the coats of the stomach?

In the experiments of Philips, the renewal of the powers of digestion was unquestionably referrible to the faculty possessed by the nerves, when stimulated by the galvanick influence, of exerting their intrinsick energies to that degree which was adequate to the execution of the proper function of the organ. A vast number of experiments on the nerves, contribute to establish the above conclusion.

So far, the reader may conclude, with great safety and propriety, that, according to the anatomical structure, the extirpation of the gland is not impracticable. But there is not a full acquiescence, among anatomists, in the simplicity of this structure; and some of the most able have attributed rather more to a division of the external carotied, and its branches, as far as they are involved, than probably was justifiable. Indeed, it is gratifying to observe, that at a subsequent period, those fears were abandoned, and the same very able author, who, as the reader will presently perceive, in his anatomy, declared the removal of the parotid gland to be impractible; at a future period, in his volume on tumours, asserts that he himself had repeatedly removed this gland, when in a diseased state, with the utmost success.

But we will let Mr. John Bell tell his own story, for certainly, any thing he writes, suffers by being put into the language of any other person.

"The surgeon's interest in understanding these arteries, is, indeed, very strong. It were impossible to enumerate all the various occasions on which this piece of anatomy may be useful; but, surely, one may easily say enough on this subject to attach the young surgeon to the diligent study of these arteries."

"Among the various motives for diligence, I would mention these; the terrible hemorrhages which he is daily called to stop, when suicides, though they have not cut the carotids, have cut the great arteries of the thyroid gland; the necessity of thinking about the gland itself, for I have had the unhappiness to see a person perish by suffocation, while consulting physicians forbade any operation, and I had no other than the melancholy privilege of watching, for many hours, the last struggles of a person, who had the day before, been walking through all the rooms in tolerable ease and health.—Could nothing have been resolved on? Must we always submit to this? Might not an incision in the fore part (where few arteries are) have at least uncovered the trachea, given a temporary relief, and made the tumour suppurate more freely? The extirpation of the tongue, which is mentioned with horrour, would be a less terrible operation to one acquainted with these arteries; the extirpation of all tumours under the jaws is dangerous; *the cutting out completely the parotid gland, is a thing quite impossible*, since the greatest of all the arteries, viz: the temporal and the maxillary, lie absolutely unbedded in the gland. What then, shall we

think of those surgeons who talk in such familiar terms of cutting out the parotid gland? Bleedings from the nose have been so often fatal, that Petit is celebrated to this day, for a discovery which he never made, viz: the way of plugging the nose, so as to stop this bleeding. Have not the French society been busy renewing inventions for securing even so small an artery as that of the dura mater? In the hare-lip operation, in cutting cancers, in dissecting tumours from all parts of the face, the surgeon commands the blood only by knowing these arteries. Cowper, the celebrated surgeon and anatomist, had his head so full of this project, that instead of waiting for hemorrhage during his operation, he cut off, two days before, the chief source of the blood. He was going to cut out the parotid gland; and two days before, he placed a small button of caustic on each side of the labial artery, where it lies upon the cheek, passed a ligature under it, tied it firm, and then proceeded to his operation next day. But this great anatomist, made, at one stroke, two grievous blunders; he missed, for want of knowledge, the chief arteries of the parotid gland, for they came from the temporal artery; and, if I mistake not, he had tied the vein, for most assuredly it is the facial vein which he is describing in his twelfth table from Bidloo. How terrible the extirpation of the tumours from the gums, throat, tensils, &c. I need not say; where the surgeon always uses burning irons instead of needles, where not unfrequently the patient dies." John Bell's Principles of Surgery.

In the anatomy of Mr. Bell, there is nothing more presented to the mind of the reader, than what grew out of the anatomical account given by the system of anatomy

of Edinburgh already quoted. But, in addition to the anatomy of Mr. Bell, his opinion is given, and that very decidedly, against the operation. Mr. Bell is one of the last writers in the world from whom the profession can feel satisfied to receive a bare opinion. The scope of his reasoning powers was so extensive, and his knowledge so copious, that whenever he fails to accompany his opinion by an argument, the reader is disappointed. And opinions are at all times those articles, which men of science should give out with great circumspection ; a chain of argument may be understood, and may convince, but an opinion, or an assertion, for the most part, leaves the understanding as unfurnished as it finds it. But, perhaps, something like a reason may be found in the following words, " since the greatest of all the arteries, viz : the temporal and maxillary, lie absolutely inbedded in the gland !" This, to an operator of the present day, might have been spared, for if in any degree acquainted with the progress of surgery, he understands that there can be no greater reason for hesitating to divide the external carotid, than the ulnar, or radial; and he would divide and take it up, almost in the same time.

No doubt, when Mr. Bell extirpated the parotid gland, as he tells us in his volume on tumours he had often done, he looked back upon this opinion expressed in his book on anatomy, with the kind of astonishment as we now do.

If there be reasons why we should not attempt to extirpate the parotid gland, they must be very different from those suggested by Mr. Bell; and some such reasons are to be found in Mr. Allan Burns, if his

anatomy, in its full extent, is to be admitted. Mr. Burns, writing on this important subject, has the following very interesting paragraphs.

"So soon as the external carotid artery has emerged from behind the stylo hyoideus and the digastrick muscles, and while it is lying over the internal carotid, it attaches itself to the parotid gland, with which, for the remainder of its course, it continues to be very intimately connected, and in the substance of which, it gives off its branches. This gland, and its connexions, are too important to be passed over with a cursory notice; its situation and extent ought to be known to every student —it is not the circumscribed and well defined gland which many believe it to be, neither is it confined to the space between the ascending plate of the lower jaw bone, and the ear. That is really a small part of the gland, the limits of which cannot be shewn by a superficial dissection, which can give no just idea of either the extent or connexions of the parotid gland. To unfold these, we must penetrate deeper; we must follow the gland to the very root of the external auditory sinus, almost to the internal carotid artery and vein; we must trace it sunk behind the plate of the jaw bone, and see it *adhering there to the internal, terygoid muscle;* we must follow it behind the sterno mastoid muscle, and down along the neck, a little below the angle of the jaw, and examine where folded over the posterior edge of the masseter muscle, and when we have done all this, we shall only have made ourselves acquainted with the mere locality of the gland. We shall have still to learn that it is while buried in the deepest part of the parotid gland, that the external parotid artery gives off the

arteria posteria auris, and divides into the internal maxillary, and temporal arteries, and likewise that it is while imbedded in this gland, that the portio dura crosses the artery.

"The parotid gland, is then sunk so deep, and is so firmly locked in between the ascending plate of the lower jaw bone, and the mastoid process, that when it becomes diseased, the patient *cannot open his mouth,* and from the effect of the fascia, the tumour is flat. *Its extirpation is quite out of the question ;* its impracticability is proved, by reviewing the connexions of the gland. Whoever has in situ injected the salivay duct with mercury, and then even where the gland was healthy, where it was free from preternatural adhesions, and limited to its natural size, has tried to cut it out, would be convinced, when he saw the mercury running from innumerable pores, that the gland extends into recesses, into which he could not trace it in the living body. If this be true in health, what must be in disease, when the parts are wedged between the angle of the jaw, and the mastoid process, and nitched into every interstice around. On the dead subject, I have attempted the extirpation of such tumours, but even there, have never succeeded in clearing away fully the diseased substance."

"The inference from this fact is too plain to require to be expressed. Those who assert that they have extirpated the parotid gland, have, I am fully convinced, mistaken that little conglobate gland which lies imbedded in its substance, and which does sometimes enlarge, producing a tumour in many respects resembling a diseased parotid, for the parotid itself. I have seen an

enlargement of the lympatick glands, taken for a diseased parotid, and the same had occurred to Mr. Cruikshank. This author when speaking of the absorbents and glands about the parotid, adds that he had known these 'indurated and enlarged to the size of a hen's egg, which gave suspicion of cancerous affection in the parotid itself.'

"This gland, in the early stage of the complaint, may be extirpated, but the parotid cannot. 'The cutting out completely the parotid, is a thing quite impossible, since the greatest of all the arteries, viz. the temporal and the maxillary, lie absolutely imbedded in the gland.' If we may credit the assertion of Mr. Charles Bell, we must believe that his brother, assisted by himself, actually accomplished this 'impossibility:' 'I assisted my brother formerly in this operation. The whole gland was diseased; it was dissected all round, until it remained attached only at that deep point, which is behind the angle of the jaw, where it encircles the artery. A ligature was put upon its root, and in a few days it dropt off, more completely eradicated, than could have been possible with the knife.' Nor does this assertion of Mr. Charles Bell rest upon his authority alone. Mr. John Bell avows the operation, which he would wish to make us believe he had afterwards performed, 'for I had afterwards extirpated the diseased parotid.' With his own words, he shall condemn himself: 'what shall we think, then of those surgeons who talk in such familiar terms of cutting out the parotid gland.'

"Did Mr. John Bell know the connexions of this gland, less perfectly, when he wrote his system of anatomy, than afterward? Did this lately acquired

knowledge, teach him that his former inference was incorrect? Did it convince him that the parotid gland may really be extirpated? Did he from this belief actually undertake and accomplish its excision with the assistance of his brother. Let Mr. Charles Bell determine the motives which induced him to 'talk in such familiar terms of cutting out the parotid gland;' and let Mr. John Bell, assign some more satisfactory reason for delivering the extirpation of this gland, than its connexions with the temporal and maxillary arteries, for these might both be controlled. The arteries are not our dread; they do not deter us from performing the operation; but the nitching of the gland into interstices from which we cannot extricate it, leave us no hope of clearing away all the diseased substance, without which, any operation would prove abortive. This is our chief consideration, and this is the insurmountable obstacle, our only barrier."

"I have endeavoured to place the question regarding the extirpation of the parotid gland in its proper light, and to shew from the anatomy of its connexions, that it is an operation, which in no situation, and under no circumstances, ought to be undertaken. If the disease be really seated in the parotid gland itself, which, in nine cases out of ten, it will not be, we could have no expectation of extirpating every particle of the tainted substance. This must decide the question. But how are the operations to be explained, in which this gland was reported to have been cut out? This will not be a difficult task. The descriptions of the operations, prove, I think, that it was not the parotid itself which was removed, but a diseased conglobate gland, of which,

there are usually two connected with the parotid. One is generally placed beneath that tube of the parotid which extends *lower* than the angle of the jaw; the other is imbedded in the very centre of the parotid, lying completely opposite to the division of the external carotid artery into the temporal and maxillary branches." Allan Burns, p. 266.

By the foregoing quotation, the reader will perceive that Mr. A. Burns, after giving his anatomical account of the parotid gland, indulges, in a strain of criticism, on Mr. John Bell, as illiberal as unnecessary. In the style of interrogation, he affects to be humorous, and assumes the tone of ridicule, while the reader most anxiously is listening for the voice of argument. Because Mr. John Bell, some years precedent, in his work on anatomy, asserted the extirpation of the parotid gland to be impracticable, Mr Burns most earnestly labours to satisfy the reader, that the assertion of Mr. Bell, in his volume on tumours, that he afterwards extirpated this gland, is disengenuous; and indirectly assures us, that Mr. Charles Bell, when he says that he assisted his brother in one of the operations, forgets the obligations of veracity. In all this, it must strike the reader, there is much more of uncourteous declamation, than strength of argument, and indecorousness of insinuation, than is either suited the subject, or compatible with the high professional standing of Mr. Burns; the reader in it is disappointed; Burns is below himself.

Now, it does not appear, at all impossible, nor indeed improbable, that when Mr. John Bell wrote his anatomy, he, upon considering the connexions of the gland with the arteries, and reviewing the surgery of

the day, might have considered the extirpation of the gland as impracticable, but at a future period, upon a more mature and deliberate consideration, joined with a more extensive experience of the measures by which hemorrhages of those arteries are controlled, he might have changed his opinion, and, upon a practical trial, found that to be practicable, which, in his anatomy, he had declared to be impossible. A change of opinion in an experimental art, does not argue either absurdity or disengenuousness. And, however highly we may value the authority of Mr. Burns, I do not know, especially as the peculiarity of the anatomy referred to by him, is mentioned by no other writer, that too much stress should be laid on this "nitching into the interstices from which the gland cannot be extricated."

That upon trial Mr. Bell found the gland "*adhering to the pterygoideus internus muscle,*" is a matter very much to be doubted. This is the first time the world has been told, by respectable authority, of the adhesion of a gland to a muscle, except under the circumstances of disease. This anatomical *discovery* of Mr. Burns, tends very strongly to weaken his general description, and to suggest to the reader great caution and hesitation, in the adoption of his views. All glands, the parotid as well as others, are uniformly surrounded by, and imbedded in, cellular substance, and Mr. Burns can scarcely be serious when he asserts that the healthy gland is found *adhering* to the internal pterygoideus muscle.—In this part of his description, much is to be attributed to the imagination, and but very little indeed to his observation, or dissection.

The descent of the lobe of the gland, to the very root of the sinus of the ear, is a matter of no insurmountable magnitude; it might even, from this situation be extricated. And as to the folding over the posterior edge of the masseter muscle, or behind the sterno-mastiodeus, the anatomical surgeon would, in all these windings and meanders, meet with but very little to call on his fears. I have repeatedly examined the parotid gland in the dead subject, and have never found those windings, and foldings, and muscular adhesions, of which Mr. Burns so confidently writes; nor are they mentioned by anatomical writers.

The tumour must be very large indeed, and hooked, in a position, of which I have no conception, "between the plate of the lower jaw bone and the mastoid process, to prevent the opening of the mouth." if such were ever the fact.

That Mr. John Bell should mistake, in his operations, the small circumscribed conglobate gland, in the centre of the parotid, for the parotid itself, is a thing very improbable; it is wholly inadmissible that the first surgeon and anatomist of the age should be thus imposed on. And it is presumable that Mr. Charles Bell, when he asserts that he himself was present, and assisted his brother John in the extirpation of the parotid gland, is quite as much entitled to consideration, as Mr. Burns is where he assures us that the tumour, when large, is so hooked in between the plate of the lower jaw bone and the mastoid process, as to *prevent the opening of the mouth;* especially if it be recollected that in the act of opening the mouth, the plate of the lower jaw is absolutely removed to a greater distance from the mastoid

process, than that at which it is when the mouth is shut.

In evidence that the operation has been successfully performed, we have the authority of Mr. John and Charles Bell, but that the gland in a healty state, *adheres* to the internal pterygoid muscle, we have the evidence of Mr. Allan Burns alone. The task of assertion by Mr. Burns, that Mr. John Bell was too ignorant to discover in his operations the difference between the small conglobate gland in the centre of the parotid, or the conglobate gland below the angle of the lower jaw, might have been very easy, but the task of satisfying the professional world, that the most scientifick, skilful, and able surgeon of his country, was thus easily deceived, is rather herculean, and requires more address and authority, than falls to the lot of Mr. Burns, however respectable he may be. The insinuation, that Mr. John and Charles Bell, laid out their operations on the broad and uncircumscribed bases of misrepresentation, is somewhat inadmissible, although the current practice of the present day, of reporters and case-makers, would, in a very great degree, justify the suspicion.

It has, moreover, fallen to my lot to be concerned in the extirpation of the parotid gland; and surely I never entered upon any operation with more serious apprehension; for notwithstanding my thorough knowledge of the anatomy of the part, which I had so often demonstrated to the anatomical and surgical classes, yet the dissection on the dead, and the cutting on the living, are very different things; to wound the internal carotid, (the external must be taken up,) or divide the branch of

the intercostal nerve, reaching up to the head, in the dead subject, would be but a light matter; in the living it might bring death to the patient. I, however, finally determined on the operation.

The history of the case is nearly as follows. About fifteen or twenty years ago, a tumour appeared a little under the ear, in a lady; the caustick was applied, under the direction of three physicians, but without success. But of late, the disease becoming much more serious, the present family-physician was called on to make an examination, and give his opinion; he advised a consultation, and being called on, I visited the patient.

The disease was too apparent, in its nature, to admit of doubt, or give room for two opinions; after a short consultation, the extirpation was decided on, and was commenced in the presence of Dr. S. Birckhead, (the family-physician,) Dr. Thomas Wright, and two of my pupils, Mr Buch. Ducket and Mr. Hays, by separating the lobe of the ear, which had become united to the tumour, in consequence of the use of the caustick, from the diseased mass. The knife was then carried up behind the ear, and backward to the mastoid process; thence it was directed downward, upon the anterior and outward line of the sterno-mastoid muscle, until it arrived about an inch below the angle of the lower jaw.—The gland was then carefully separated from its posterior relations; and this being accomplished, I next passed over to, and upon the masseter muscle, keeping at a proper distance from the diseased part, and ascended to the zygomatick process of the os malæ; and then along that, rather above the lower border, to the ear.

Upon the accomplishment of this, I proceeded on with the dissection of the morbid gland, from its attachments over the surface of the masseter, and down along the plate of the lower jaw. By the latter part of this dissection the internal maxillary and transversalis faceii arteries were divided; these were secured. Having thus got the mass pretty well detached behind, from the course of the sterno-mastoid, and in front from its relations to the masseter and plate of the lower jaw, I cautiously approached the external carotid, on each side and in front; the artery thus insulated or nearly so, could with great facility be commanded by passing behind it a crooked needle armed with a ligature; after being tied, it was divided.

The real danger was now to be encountered; the tumour was to be separated from its attachments, towards the internal carotid artery, and the branch of the great sympathetick or intercostal nerve as it ascends to the head; and indeed, had the tumour stretched far in upon the internal parts of the throat, the internal jugular, and nervus vagus, would have been endangered. In getting along the inner surface of tumour, and passing between that and the parts just enumerated, I conceive to be the only part of the operation in which there is hazard: the tying the external carotid, before it is divided, about the angle of the jaw, and again where it is entering the temporal region, will occasion no trouble, to a hand accustomed to dissection and taking up arteries, and none other, assuredly, should engage in extirpating the parotid gland. Yet, by laying hold on the lower point, after the artery is divided, just above the

main ligature, and drawing the mass out, the operator may dissect, if he is cautious, in safety.

Time, in this operation, is not to be too carefully measured; the loss of a few minutes, is of little moment; the sponge must be applied; and the parts well cleansed, at every instant of time—sat cito, si sat bene.

In the above operation, I was very much aided by the kind attentions of Dr. S. Birckhead, and of Dr. Thomas Wright; the latter of whom was so polite, as to lay the ligatures for me in his usual collected and steady manner. I felt also indebted to the two young gentlemen, Mr. Duckett and Mr. Hayes, students of medicine in my office, for their prompt civilities. The lady is recovered, and perfectly well, with the exception of some obliquity of the muscles of the face, owing to the necessary division of the portio dura of the seventh pair of nerves, upon which the muscles about the eye and face, very much depend for their sensibility and action. This little deformity is, perhaps, irremediable, but is of little moment, when put into comparison with the advantages of the operation—the security of the life of the patient.

The parotid gland has been repeatedly extirpated by the Germans, as Hiester informs us.

PROFESSIONAL USAGE

JUSTIFIED BY ETYMOLOGY.

BY LINACRE.

Tu enim, Cæsar, civitatem dare
Potes hominibus, verbis non potes.

A QUESTION has been started, of late, on the professional import of the terms, infection and contagion; whether they be synonymous; or whether, by the former, we synonymise the latter, and vice versa. They are both derivative words; in their etymology Latin, and, like the word inoculation, in their professional application, metaphorical; and perhaps the discussion is better calculated to amuse the reader than to instruct the scholar. Yet I may be indulged for a moment or two.

"Contagion," says Dr. Johnson, "is the emission from body to body, by which diseases are communicated; is *infection*, propagation of mischief or disease; pestilence, venomous emanations."—Dictionary.

"Contagion is derived from the latin substantive contagium; and this again from the verb contingo; and ultimately from con and tango, to touch, to hit, to arrive at." Ainsworth.

To touch, to hit, to arrive at, is, by some, interpreted, that the two bodies—the healthy and the diseased—shall mutually come into absolute contact: that is, surface shall touch surface. But this interpretation is by no means consecutive or logical. To touch, to hit, to arrive

at, by fair construction, in no degree, refers to the immediate touching of human bodies, but, professionally interpreted, to the poisonous emission from the diseased body coming into absolute contact with the living principle of the healthy body. And further, that not only does the poisonous emission come into contact with vital principle, but must, more or less, impress on the vital principle, its own laws, or communicate to it its influence, and thus produce an affection, in phenomena, similar to that from which it is derived, in the diseased body.

The English verb, to infect, is from the Latin, inficio, to dye, to stain, or to colour. In a technical, or professional sense, to dye, to stain, or to colour, is to impress, or communicate influence, or, more or less, to subject the principles of life to a certain rule or dominion. It cannot be that the poison really stains or dyes the vital principles with any colouring matter, but that it makes a certain impression, to be recognised in its evolution by regular sensible phenomena.

Now, a disease to be infectious, must so influence the affected body, as for its emissions to subject to their law and control, the vital principle of a healthy body.—And, except the emissions, or excretions, so far impress their own nature on the healthy, as to produce a sort of phenomena similar to those exhibited in the diseased body, the disease cannot otherwise, logically, or with any propriety, be termed an infectious disease; or, be said to dye, or to stain the healthy body.

When a disease is produced by a contaminated atmosphere, and is not re-produced by an intercourse, or commerce, between diseased and healthy bodies, such atmosphere may be strictly denominated infectious, or,

and particularly of the naval and military hospitals," from which have originated all the modern improvements in the French system of medical education.

The fine building of the late academy of Surgery was appropriated to l'Ecole de Santé at Paris, which became the centre of medical as well as surgical instruction.

The *Convent des Cordeliers*, celebrated on account of the Jacobinical meetings held there at the time of the revolution, was converted into an hospital (*l'hospice de Perfectionnement*) for the purpose of receiving extraordinary cases, and making such experiments with new remedies, as might tend to the advancement of medical science. Dissecting rooms were ordered to be built, to each of which a *prosecteur* was appointed to direct the students, and to repeat to them the subjects of the last lectures of the professors. Bodies for dissection were to be furnished from the hospitals, the professors merely specifying the number required.*

By the decree of the *Convention Nationale*, twelve professors were appointed to "l'Ecole de Santé" at Paris, as many assistant-professors, a director, inspector, demonstrator in chief, librarian, conservator of the museum, *dessinateur-peintre*, modeller, &c.

The following were the lectures to be delivered.—During the winter six months—1.—Anatomy and Physiology. 2.—Medical Chemistry and Pharmacy.—3.—Operative Surgery. 4.—Doctrine of Hippocrates,

* The regulations for the establishment of les Ecoles de Santé contain a clause that "*toutes les prescriptions se feront en Francois*" —and it is still the general practice in France to write the prescriptions in the French language.

and history of rare cases. During the summer—— 1.—Medical Natural History. 2—Doctrine of Health. 3.—Internal Pathology. 4.—External Pathology —— 5.—Medical Jurisprudence, and History of Medicine. 6.—Midwifery, two courses; one of four months for *les eleves de la Patrie;* the other of two months, for women intending to become midwives. During the whole year—1.—*Clinique interne,* or medical clinical lectures, at *la Charité.* 2.—*Clinique externe,* or surgical clinical lectures, at *l'Hôtel Dieu.* 3.—Clinical lectures, at *l'hospice de Perfectionnement.* All the clinical lectures were to be given at the same hour; but the other lectures were so arranged, as not to interfere with these, nor with each other.

The great object in establishing the three *Ecoles de Santé,* was the education of national students (*les eleves de la patrie.*) From each district in France, young men, between the age of seventeen and twenty-six, were sent to receive medical instruction at one of these three schools, free of expense, and the number of them to be educated at l'Ecole de Santé of Paris, was fixed at three hundred. These national students were divided into three classes, according to their advancement.

The commencing class was to follow, during the winter, the courses of anatomy, physiology, chemistry, and demonstrations of objects in the museum; during the summer, to assist at the lectures on medical natural history, demonstrations of the bones, and application of bandages.

The second class was to follow, in winter, practical anatomy, chemistry, operative surgery, and the doctrines of Hippocrates; in summer, natural history, external and internal pathology, and midwifery.

The first class was to assist the professors at the winter courses of lectures on anatomy, chemistry and operative surgery; and in the summer to attend to medical natural history, external and internal pathology, history of medicine, and medical jurisprudence.

During the whole year the national students were to attend the clinical lectures of the different professors, and the three classes attended in succession, for four months, at each of the hospitals. The *clinique externe*, at *l'Hôtel Dieu*, was appointed for the third, or commencing class; the *clinique interne*, at *la Charitê*, for the second; and the *clinique de Perfectionnement*, for the first.

To secure the attendance of the national students at the hospitals and clinical lectures, each professor was directed to call over their names twice in ten days, and those who were found absent three times out of ten, were referred to the committee for regulating public instruction, and reprimanded or dismissed.

To ascertain the progress of the national students, at the conclusion of each course, those amongst them who had followed it were assembled, and three questions proposed, to which they gave a written answer at the end of an hour and a half, from which the professors judged of their advancement. Besides this, there was held, annually, a general examination of all the students, when premiums were given to the most worthy, and those who did not satisfy the professors, were discharged to make room for others.*

* From the increase of the French territories, and the necessity for a greater number of medical officers to supply the army, three

Such was the system, under which l'Ecole de Santé (since called *l'Ecole de Medecine*) at Paris, for the gratuitous education of medical officers of the hospitals and the army, was commenced and carried on; the lecture rooms and hospitals were open to the public, and those eager to get instruction, found the greatest facilities of obtaining it; but there was no school, or college for the reception of students, desirous of following a regular education, in order to become private practitioners, without entering as "*eleves de la patrie.*"

In 1803, a law was passed, by which les Ecoles de Santé of Paris, Strasbourg, and Montpellier, were made the colleges for granting degrees in medicine and Surgery, under the regulations which are still observed by those who wish to graduate, and are not national students. From the date of this law, no one could legally become a doctor, nor practise as a surgeon or physician, without undergoing a course of study of above four years —yet, from negligence on the one side, or imposition on the other, the whole has been completed in six weeks! Mischiefs as great arise from the abuse of laws, as from the want of them; and, it would be better to have no medical or surgical colleges in France, England, or any other country, than to have men practising under the authority of a degree, or a diploma, that has been obtained by stratagem, or conferred without discrimination.

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It could hardly be supposed that the regularly educated doctors of medicine, or surgery, would settle in small towns; and therefore the above law provided against this evil, by appointing, as country practitioners, men of less extensive education, under the title of "*Officiers de Santé,*" who were not required to study at the great schools of Paris, Strasbourg, or Montpellier, but were licensed to practice, after living six years with a regular doctor, or five years in a country hospital, and being examined by a jury of medical men, appointed for that purpose in each of the departments. These *Officiers de Santé* were allowed to practise only under certain restrictions—they could not perform the grand operations in surgery, unless under the *surveillance* of a doctor, or in cases of emergency, and they could not exercise their profession out of the district in which they had been examined.

This outline of the principal laws that have been made since the revolution, to regulate medical education and practice in France, will render some parts of the following sketches more easily intelligible, and spare much repetition. The history of all great institutions is interesting, particularly when the foundation of them embraces plans altogether novel and peculiar; and so much has been written in France upon medical education, that I could readily extend my account of laws, and abuses of laws, to a great length; but my time will be better occupied in exhibiting the actual state of medical instruction at Paris, by a description of what has come under my own observation.

L'Ecole de Medecine is the only institution in Paris, for granting degrees in medicine or surgery, to those who have gone through the requisite forms of study; it is supported at the expense of government, and forms, with the hospitals connected with it, the universal school of public medical instruction.

The courses of lectures delivered last winter, at l'Ecole de Medecine, from November to April, were

1. Anatomy and Physiology, daily at ten.
2. Medical Chemistry and Pharmacy, three days in the week, at twelve.
3. Operative Surgery, on the three intervening days, at the same hour.
4. Demonstration of Surgical Instruments, every Thursday, at one.

The professors at l'Ecole de Medecine have salaries from government, and all the lectures delivered by them are public—every body can get admittance without paying any fee, or submitting to any ceremony. Those who are found at the lectures, may therefore be divided into three classes.

The first class is the least numerous, and includes those who attend through curiosity, or from a desire of gaining information, without intending to obtain a degree, or to go through a course of education that shall enable them to become candidates for any medical appointment.

The second class, are those students who are pursuing their education to obtain the degree of doctor of

medicine and surgery, for each of which the same education is required. They are obliged to take *inscriptions*, or tickets, for all the lectures, which occupies about four years, as they can be inscribed for one course only at a time, and take a fresh *inscription* but once in three months. The expense of being inscribed is from twenty-five to thirty shillings for each course, so that the sum paid for all the inscriptions during the four years, amounts to little more than twenty pounds. These students have no privileges over the first class, except being able to ask for a degree at the end of four years. They must be present at the commencement of every three months, to take a fresh *inscription*, but their attendance on the lectures delivered at *l'Ecole de Medecine*, or at the hospitals, is voluntary, and the only inducements for them to attend, are the examinations they are obliged to undergo, previous to being admitted to their doctorship. The examinations are five in number; all of them are public, and two of them are conducted in the Latin language The first examination is on anatomy and physiology; the second, on pathology and nosology; the third, on materia medica, pharmacy and chemistry; the fourth, on *hygiène* and *Medecine legale:* these are the same, whether the candidate wish for a degree in medecine or surgery. For a degree in medecine, the fifth examination is on "*clinique interne;*" in surgery, on "*clinique externe.*" Besides these examinations, the candidate has to write a thesis on some medical or surgical subject, and to defend it in the public theatre. The expenses for the examinations are the same for either degree, and amount to about twenty-five pounds (five hundred *francs.*)

The income arising from the *inscriptions*, examinations, and reception of this class of students, is divided amongst the professors, and forms an eventual salary, which they have in addition to the fixed salary (five thousand *francs* per annum) received from government. The more respectable students, who have some property at their disposal, follow this course of education; and notwithstanding the imperfection in one respect, that there is no adequate provision against the neglect of attendance in those who take their *inscriptions*, the examinations are so good, and so well conducted, that few young men, I am inclined to believe, present themselves to obtain the degree of doctor, without being well informed of the profession which it authorizes them to practise.

The third class who attend the lectures at l'Ecole de Medecine cannot be understood, until I shall have explained what is meant by *l'Ecole pratique*, of which they form a part.

L'Ecole pratique, includes the professors of l'Ecole de Medecine, the surgeons and physicians of hospitals, demonstrator *en chef*, *prosecteurs*, and assistants of the anatomical school, and what were formerly called national students, or *les eleves de la patrie*, who are practically employed as assistants to the professors, in the dissecting rooms, or in the hospitals. These students of *l'Ecole pratique*, are received after an examination *au concours* by the professors; they are examined regularly, to shew their progress in their studies, and are admitted to be candidates for annual prizes which are bestowed on the writers of the best dissertations, &c. Besides these means to insure their attention

to their studies, and incite them to make great exertions, they have other advantages ; they are exempted from paying for their *inscriptions*, as well as for their doctor's degree in medicine, or surgery, which is given to them, gratis, after the regular course of study, and the examinations which I have explained in speaking of the second class.

These *eleves de l'Ecole pratique*, therefore, compose what I mean by the third class of students, to be found at the lectures of l'Ecole de Medecine. They are students who have little money to pursue their education, or who are intending to become medical officers in the army and navy, to which they are admitted after a longer or shorter course of study, according to the need of them, or the proofs they give of advancement in their profession, without having taken their doctor's degree.

It appears an admirable system, that renders instruction upon a science so noble and useful as medicine, accessible without expense ; but it is a system open to many objections. What is gotten for nothing, is very little valued, and professional rank is lowered in proportion as the expense of attaining to it is diminished. The French certainly have put in execution, a plan which has often been speculatively proposed by others, in prescribing the same course of study and examination to the physician and the surgeon, and in granting to each the degree of doctor, and equal privileges in practice ; yet, in doing this, they have rendered the two branches of physic and surgery less distinct. A doctor often exercises, in France, both physic and surgery, although

ordered by law to confine his practice to the branch of the profession in which he has taken his degree.*

There is another thing besides the cheapness of pursuing medical education, which has contributed to increase the number of students in France, and overload the public with medical practitioners. Those who had followed a certain course of medical studies, and were still following it, were exempt from the *conscription;* and therefore young men, at sixteen years of age, began for this purpose, to study a profession, which cost them almost nothing, and saved them a great deal.

The students of *l'Ecole pratique* are received with too little attention to their previous general education; full scope is however given to young men of talent, and all impediments from obscurity of birth or want of fortune, removed. Why then do we not find the medical men of France far outstripping those of other nations?

The students of *l'Ecole pratique* have a great privilege in being afterwards enabled to become candidates for the higher offices in the school, to none of which are those eligible who were merely common students, and paid for their *inscriptions.*

The manner in which all the officers of l'Ecole de Medecine are elected, is so excellent, that it would counter-balance a thousand objections against the whole

*Doctors in medicine or surgery are alike prevented from dispensing medicines; they can therefore only practise in cities or large towns; and the inhabitants of the country are entrusted to the *Officiers de Santé*, who, as I have already explained, may have their licenses without going out of the district in which they were born.

present form of medical education in France; and I shall endeavour to set forth this point of excellence, by explaining the method in which the office of * *Chef des Travaux Anatomiques*, not long ago vacant, was bestowed. It will be an example of the plan, according to which all the appointments of *Prosecteurs*, *Aides d'Anatomie*, &c. are conferred.

The office of *Chef des Travaux Anatomiques*. was declared vacant by public advertisement; every person of twenty-one years of age, or upwards, who had formerly been received as an *eleve de l'Ecole pratique*, was invited to become a candidate; and the list of candidates was closed fifteen days before the commencement of the public *concours*. A jury of seven of the professors of the faculty of medicine was to decide on the merits of the candidates, and confer the appointment. The candidates, who were four in number, submitted to three methods of trial, by some of which, each might shew the extent of his acquirements.

First trial, by giving written answers to questions proposed by the professors. The first thing required was to explain the anatomy and physiology of the ear, and six hours were allowed the candidates to prepare their answers. The second, to be answered in four hours, was a description of the organ and sense of smelling. Five days were given to each of the candidates to answer the third proposition, "to take a general

* This situation is next to a professorship, and two of the most eminent professors of the '*Faculté de Medecine*" at present (Dumeril & Dupuytren) were in succession "*Chefs des Travaux Anatomiques.*",

survey of the duties of the situation they were contending for, in order to render it at the same time useful for the instruction of students, and the advancement of science."

Second trial, by making anatomical preparations, and performing operations in public. For the former purpose, each candidate was requested to present, at the end of fifteen days, the following preparations: 1. The internal maxillary artery. 2. The vena azygos, subclavian veins, and the lymphatic trunks, which empty themselves into the latter, injected with solid matter. 3. The course of the great sympathetic nerve on one side of the body. The public operations required to be performed were: 1. For fistula lachrymalis by incision into the sac. 2. Amputation at the shoulder joint. 3. Partial amputation of the foot according to Chopart's method.

Third trial, by verbal answers to questions, or by lectures on proposed subjects. Twenty minutes were granted to each candidate, to give a short demonstration of the anatomical preparations made by his competitors, and to offer what critical remarks he chose upon them; and twenty minutes were also allowed, for each to give a lecture on "the nasal canal, the diseases to which it is liable, and the operations which may be required to relieve them."

All these *sujets d'epreuve* had been drawn by lot, from a considerable number of others, proposed by the professors who composed the jury; and the contest lasted for above two months.